Journal
of
The Franklin Institute
Devoted to Science and the Mechanic Arts

Vol. 269 MARCH, 1960 No. 3

THE SCIENCE OF YESTERDAY, TODAY, AND TOMORROW*

BY

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INTRODUCTION

It is within a period of less than three quarters of a century, a period less than the span of life which many of my audience have experienced, that one of the world's most eminent men of science volunteered the idea that the discovery of nature's laws was ended, that the brain of man had solved the riddle of the universe, and that science was dead.

It is true that there were a few unopened, or partially opened, boxes which had come to light, and which seemed to contain things of some interest, but it was generally supposed that these things were, in principle, much the same as the things to be found outside. The contents of the boxes seemed to be in rather a messy state. However, it was generally believed that if they were cleaned up and put in order, they would reveal nothing new. The job of cleaning them up seemed to invite little interest, so for the time being, they were left as they were.

And so, even as the great surgeon, having performed his major operation, leaves to a humble assistant the task of cleaning up and removing the stitches, so the man of science felt that his work was done, and that he might leave to lesser lights the task of polishing up the contents of those boxes and of finding out how they fitted together to useful ends.

But when the boxes were opened, it was found that many of them contained things of a nature quite unexpected. The things which were in the boxes did not behave according to the common sense of the day.

* The Charles S. Redding Lecture, delivered at the Stated Meeting on December 16, 1959. Dr. Swann was honored at this meeting for his thirty-two years as Director of the Bartol Research Foundation.

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They had to do with such phenomena as gases excited to emit light by electric discharge. By and large, they had to do with glowing things. If in those days there had been any radio tubes, they would undoubtedly have been found in the boxes, together with all the paraphernalia of modern electronics. Indeed, many of the materials necessary to bring to light this important realm, were there, but they were constructed on a scale too small to be perceived by the eye of man, so they passed notice as useless debris. In this debris would have been found the substances out of which today we make transistors which supply your hearing aids. In the boxes would have been found things which, by proper assembly, would have produced X-rays. Some of them would have contained substances like radium and all the multitude of atoms which today we know as isotopes; or at least, they would have contained the wherewithal to produce these things which today play such an important part in medicine and industry. In one big box there would have been found the sun itself uttering complaints that the man of science had given him no guarantee that he would be able to go on emitting light practically forever, that the physicist had provided no security for the maintenance of his bank account of energy, and that without it he was in danger of degenerating into celestial bankruptcy.

In those boxes would have been found all the ingredients necessary to produce atomic bombs and provide for the release of atomic energy in general. In them would have been found, in a form too small for the eye to see, all the mechanisms necessary to provide for the doings of the greater universe, for the behavior of the stars and the great galaxies of space with all their mysteries, including the continuous production of cosmic rays, and the like.

Truly, those men of science of three quarters of a century ago, who left those boxes to the care of underlings for the unravelment of their contents, as the surgeon leaves to his assistants the task of cleaning up the patient, truly these great men of science died with a huge, if unknown, responsibility upon their shoulders. They are almost fortunate in having died before they were suspected of having left so much unfinished while they had declared that all was, indeed, finished.

Since man attained the stage of mentality in which he felt the desire to think about himself in relation to his surroundings, he acquired the ambition to *understand* nature. The basis of such an understanding is an elusive thing. It is by no means obvious. To put the matter in a nutshell, we may perhaps say that, in the past, to understand has been, for man, the ability to see in new phenomena which he studies, nothing more than the operation of the same principles as he has already accepted in the things which he has previously studied.

THE SCIENCE OF YESTERDAY

And so, in the beginning, the things which man learned to accept were the behaviors of beings like himself. Thus, in order to understand

how he and his surroundings could be controlled from the outside, he invented beings like himself who, indeed, had the power to control these things, even as he and his fellows had, on a smaller scale, power to control those who served them. Reversing the policy cited in Holy Writ, man invented these omnipotent beings in his own image, and with many of his own vices and shortcomings, as well as with his beneficent characteristics. The gods were angry, and they hurled thunderbolts. The gods were pleased, and they showered the earth with the blessings of spring. Anger and pleasure are such common attributes of mankind that they seem to call for no explanation in themselves. As regards a wider range of characteristics, the capriciousness of the gods, the uncertain temperament of the gods, and so forth, man, in seeking a basis for the acceptance of these things as normal, had to do no more than think of all the primadonnas of his age, and indeed of all the ladies of his own acquaintance. Alas, in those days there were no psychiatrists to analyze man's emotions as the outcome of more fundamental "causes"; and so mankind was content to "understand" in terms of the laws which governed his primitive feelings and experiences.

Early in his history, homo sapiens became conscious of the efforts of his muscles, and the need for exertion in order that things should be accomplished. To bring stationary things into a state of motion, man found that he had to do something; and in the doing of it he became conscious of effort, so that there arose a vague concept of force. However, to push anything and make it move, one had to come into contact with it. A man could not, by merely flexing his muscles, cause something at a distance to start moving. The force had to be transmitted from point to point in order to become effective. As a matter of fact, insofar as there is any difficulty in understanding motion at all, there is just as much difficulty in understanding it through the transmission of force from point to point in a medium as there is in understanding action at a distance, The late Sir Oliver Lodge once remarked that it is as yet an inexplicable fact that when one end of a rod is pushed, the other end moves, to which observation, PUNCH replied: that it is also an inexplicable fact that when one end of a man is trodden upon, the other end shouts. However, the layman readily accepted the philosophy that what the eye cannot see the mind need not trouble about. And so, the transmission of force through minute distances seemed to present much less of an obstacle than did its transmission over great distances.

The Aether, and the Transmission of Force

The motions of the heavenly bodies became explained in the hands of Newton as motions which should be thought of as caused by "forces" whose origins were in the heavenly bodies themselves; and while Newton himself would probably have taken a more philosophic view of the

meaning of this statement than would many of his followers, those who wished to "understand," and had faith in the meaning of such understanding, felt, for the reasons I have already stated, unhappy about the acceptance of such a philosophy. They demanded some kind of a medium permeating all space between the heavenly bodies, a medium which could transmit the desired force. Later, this medium was charged with the duties of every conceivable kind of phenomenon by which one body appeared to influence another body at a distance. It became charged, among other things, with transmitting the light and heat of the sun to earth, and later it was charged with the transmission of radio waves. It was natural to try and understand this medium as something like a solid or a liquid, or a gas of our common experience, but alas, the demands on it did not harmonize with any of these characterizations. And so this medium, this "aether," as it was called, remained as a mystery. As long as one did not inquire too much about the mechanisms of its activities, it served as a balm to the conscience of common sense in seeming to relieve us of the terrors of action at a distance. Many were the attempts to provide inner mechanisms by which man could understand, in terms of the common sense of the day, all that seemed to be happening; but the mechanisms for different activities were inconsistent and all that remained was the apparent potentiality of transmitting something from one place to another with a finite velocity, even though one did not know what the something was which was transmitted. It was with this dilemma in mind that, some years ago, I defined the aether as a "medium devised by man for the purpose of transmitting his misconceptions from one place to another." It was during the period of prohibition, and I added an observation to the effect that "of all subtle fluids invented for the stimulation of the imagination, it is the only one which, so far, has not been prohibited." Later, alas, it also became prohibited, when the theory of relativity came upon the scene, declaring that it had no substance in reality, was inconsistent in philosophy, and was a useless encumbrance to the brain which tried to use it.

And so, it came about that insofar as it was meaningful to speak of one body as "acting upon" another, one had to accept "action at a distance" as something which, while dubiously respectable, was not a thing to be talked about in polite scientific society. I think, however, that we must realize that with his banishing of the aether to the realms of nonsense, man took one of his first steps in removing his ideas from the realm of popular understanding in terms of the everyday experiences of the times.

The Principle of Predetermination

In this evaporation of some of the elements which were part and parcel of the intuitive thinking of a hundred years ago, there yet re-

mained one principle which man was loath to discard. This principle invoked the idea that, at any rate as regards the inanimate world, that which is happening now determines that which will happen just a little later. And that which happens a little later determines that which will happen still a little later, and so on, ad infinitum. It is the principle of predetermination. It had its most explicit exemplification when science, through the activities of Newton and his contemporaries, described the motions of the heavenly bodies in terms of the now well-known laws of astronomy. The concentration was on what we call the laws of motion of the bodies. These took the form of what are called the differential equations of motion. However, it is sufficient to say that these laws were such that, if you specify what exists now, they tell you what will be found just now—at the next moment, that is—and so on, ad infinitum. If you asked what must be expressed now, the answer, in terms of Newton's laws, is to the effect that you must assign a position and velocity for each one of the bodies whose motion you wish to discuss. In terms of these positions and velocities, the future is determined completely in terms of the present. In order that you should not derive too much comfort from this statement, however, I must remark that if you should specify the positions and velocities of the bodies a thousand years hence, those laws will equally well serve to determine where they are now. It is hardly polite to destroy your comfort in the belief that the present determines the future by asking you to accept a doctrine to the effect that the future also determines the present. I once worried myself about the problem of why, if I am to understand memory in terms of the present as determined by the past, I cannot also remember the future, if the laws work both ways.

The great success of the classical astronomy of Newton and the discovery of the atomic nature of matter and of the fact that the atoms themselves are composed of what we call particles, made it almost inevitable that man should try to understand atoms and their doings in a crude way by picturing them as models of the solar system itself on an enormously reduced scale; and so, some three quarters of a century ago, there arose atomic theories based on this idea and carrying with them, therefore, in principle, the laws of predetermination. Perhaps I should pause for a moment to state what, in the last analysis, is indicated by the acceptance of such a principle. The matter is illustrated by the story of a man and his slave.

It appears that there was an ancient noble whose belief in predetermination was very firm. And the noble had a slave who stole some of his master's possessions. For this sin, the noble made preparations to chastize his slave. However, the slave, being of a wily and ingenious disposition, said: "My master, you must know that I am not responsible for this sin which I have committed; for according to the philosophy to which you subscribe, it was pre-ordained that I should steal this, your

possession." However, the master replied: "Yes, my slave, that is indeed true; but by the same token, it was also pre-ordained that I should beat you for your offense." I commend this principle to those who have charge of the destiny of youthful delinquents.

Anyone who subscribes to the principle of predetermination, and who is confronted with a situation in which a system suddenly departs from the course predicted for it by the laws assumed, would have but two views of the matter open to him. He could deny the truth of the laws, or he could regard the occurrence as a miracle. This is, indeed, no more than a crystalization of the meaning of the word "miracle."

THE SCIENCE OF TODAY

The Miracle of Atomic Science

Now, in opening up the boxes of which I have spoken earlier, it was found that those things therein which were pertinent to the structures of atoms and molecules do not behave according to the principle of predetermination. They do not behave according to that smooth running of things which science had come to idealize. Every change which the atom experiences is a sudden one, with no clear-cut relation to the past, and no promise as to the future. Every change is a miracle in the sense in which I have sought to define that word. Moreover, it seemed, to most physicists, impossible to devise any laws consistent with the facts and according to which changes in atoms and in the realms immediately dominated by atoms occurred in any strictly predictable manner. The best that could be done was to invoke the concept of averages and to devise laws which told the chance that any particular occurrence would happen under certain assigned conditions. The laws were analogous to those which the insurance specialist uses when he predicts the fraction of all the people over, say, 50 years of age who will die in the next year. He cannot predict what will happen to any individual, but he can predict with considerable certainty what will happen to groups of individuals. In a sense, we may say that the whole quantum theory of today is a crystalization of the best laws which man has been able to devise for describing the nature of miraculous happenings. Of course, you may well say that if the insurance man should consult the physician of each individual and should order continual tests to be made of the state of health of each individual, then he could approximate with some certainty to accurate predictions as regards the individuals. You may say that there is really no miracle about this matter. His uncertainty as regards the individual is simply something founded upon his ignorance of the complete story and of the impracticability of supplementing his knowledge to the end of making detailed predictions. And so you might think that the same thing would apply to the atom, and that, if you would only work hard enough to invent a more complete set of laws to govern its actions, the complete life history

of every atom would be known and there would be no need to invoke miracles. However, physicists have worked very hard in an endeavor to do something of this kind, but without success, and the nature of their thinking is such as to convince many of them that complete success could never be attained, and that, as regards the atomic realm; we shall always have to put up with miracles. And now we are confronted with a curious psychological paradox. The average man of science, secure in the conviction that, as regards matter in bulk, nothing miraculous ever happens, is perfectly content to accept such happenings in the atomic world. Miracles on the scale of size of anything which we can see would be an abomination to him, but happenings which he cannot see, which the mind can only think about, but which he believes to occur, are acceptable. However, he avoids clash with his conscience by refusing to give them a name. Perhaps a still more curious thing is that with the advance of experimental techniques, man can actually observe certain of the miracles of the individual atoms; but here he feels his activities so far removed from anything to do with mankind that again his philosophical conscience makes no protest.

I feel it quite safe to say that if I should describe to any intelligent layman who was unacquainted with mathematical physics the principles according to which our so-called laws of atomic and nuclear structure operate; and if I could get my message across in a very short time, so that the layman would not become inveigled step by step into this way of thinking without encountering at each stage more than his philosophic conscience could swallow, I think that if I could do this, the layman would have to admit that the occurrences permitted in nuclear physics are, in terms of his normal criteria of common sense, more abstract and bizarre than any occult phenomenon which had been said to have occurred and which, under that name, he would probably dismiss immediately as evidence of insanity in those who subscribe to it.

During the last three quarters of a century, science has brought forth many marvelous things which seem commonplace today and which have not startled mankind unduly at any stage of their development because their development has come upon us gradually. If, a hundred years ago, someone had awakened in the morning to find in evidence an apparatus which enabled him to hear the voice of a man speaking in Paris; if, as he listened, he saw an airplane overhead, and if, on going into the street, he found vehicles dashing about without the aid of horses, he would surely think that he had come upon an age of miracles as remarkable as any of which he had read in the past. However, these things are no longer miracles to him because the scientists have told him that they know how it all happens; but when he gets down to ultimate fundamentals, even the scientist himself has to base his understanding upon processes which, if he could suddenly convey their nature to the layman, would have to be regarded by that individual as miracles in terms of his natural criteria of common sense. It is the miracles of the

atomic and sub-atomic world which determine the activities of things on a larger scale, where their activities come to the attention of all of us, as symbolized by that docile entity "the man in the street." This man hears of the atomic bomb, so like an enlarged version of one of the urns of the Arabian Nights, urns from which, as the result of proper incantations, terrifying beings emerged. He learns that two apparently inert pieces of uranium of the same kind, on being brought suddenly into close proximity, explode in a manner such as to emulate all the furies of hell, pouring forth all sorts of evil things in the form of poisonous radioactive radiations and the like. It is as though these two pieces of metal, on being brought together became infuriated by each other's presence and, in their anger, revealed all the evil that was within them. Indeed, from the standpoint of over-all results, the performance of these two innocent pieces of uranium surpasses, in immeasurable degree, all the mysteries described in the immortal book of Arabian fairy tales. And our man in the street, on witnessing the atomic bomb, might well say "Here, at last, I find a real miracle—a miracle which can be repeated at will." But the men of science tell him that they know all about what has happened and that there is no miracle. In this they play some deception on that layman, for, if they could reveal to him the picture of those more subtle atomic processes which are involved, he would be likely to exclaim "But these processes in terms of which you explain the bomb are, to my way of thinking, miracles themselves." And the man of science, if honest with himself, will have no choice but to reply, "Yes, my friend, that is indeed true to your way of thinking; but to me, who has lived with these sub-atomic phenomena so long, the phenomena have ceased to carry with them the stigma of the word miracle. And so," says the man of science, "I ask you to be content in my statement that all is really well in the philosophy of the matter. Then you will not be worried unless you think too much. I shall be content on account of the fundamentality of my knowledge and the broadness of my philosophy, while you shall seek refuge for contentment in the depths of your ignorance."

And so, after a time, the man in the street learns to regard the behavior of the atomic bomb as something not too much to be marvelled at, and he accepts it as he has accepted radio or as, at an earlier time, he had accepted the ordinary phenomena of electricity, the running of street cars as the result of something peculiar happening in copper cables which, by some mysterious means, are said to transmit electric power. He accepts these things as in a still earlier epoch he accepted the motions of the heavenly bodies as phenomena not to be denied, phenomena familiar in the experience of all, but phenomena which did not seem to weld together with the idea of action through contact, which the naive intuition of the day seemed to regard as a natural haven of contentment in the understanding of all things.

Now, in spite of all I have said to persuade you that we live in a world of miracles, you will perhaps be unhappy about my definition of that term. You may prefer to regard a miracle as a thing of such unusual occurrence, that the fact of its having occurred at all is open to doubt. You may then maintain that atomic phenomena are not miracles because they are always occurring, and their continual occurrence provides, in its totality, for the phenomena evident around us. If you say this, I fear that the Lord hath delivered you into mine hands; for in this sense, practically all the phenomena of the atomic world would indeed be miracles to any supposed inhabitants of the atom.

Consider the emission of an X-ray from an atom. Even if, in imagination, you lived on one of the atoms which compose the part of the X-ray tube from which the X-rays come, so rare would be the emission of a ray from an individual atom that you would be put in an atomic lunatic asylum if, as a resident of such an atom, you maintained that any such phenomena had ever occurred. Only because there are so many atoms does the physicist observe a strong emission of X-rays from the X-ray tube. And so, what is a miracle to the resident of the atom is no longer a miracle to him who observes a multitude of atoms. A similar remark applies to practically every phenomenon in atomic physics.

A cosmic ray, passing through this room, detaches an electron from an atom here and there, and by observing this phenomenon we investigate and measure the rays. Yet, to the individual atom, this theft of an electron by a cosmic ray is such a rare event that the chance of its happening to any particular atom in the period of, let us say a day, is no more than the chance that one of you would be murdered in that day if, with the earth at its present population, only one murder were committed in three hundred years.

And so it is with all the happenings of atomic physics. And yet it is these miraculous happenings which, in their totality, produce all the interesting things which our coarse-grained senses observe. And to these coarse-grained senses there is no miracle, everything happens smoothly with apparent certainty of prediction.

Harmonization of Different Domains of Science

So far I have concentrated on laws and phenomena associated with what is customarily called the realm of physics. Even here I have to admit that theoretical physics is at present in rather a messy state. When, however, we contemplate the wider realms of knowledge embracing biology and what we have recently learned in astronomy, there is much to be desired. Our knowledge of nature is like that of a world of little islands and countries, separated from each other, each being governed, apparently, by its own laws, with no very satisfactory relationship between the laws of one country and those of another. In the

affairs of men one can tolerate a situation of this kind. One does not expect the laws of all nations to agree, although one has a hope that in time they may. In science, however, we have sufficient respect for the design of the universe to believe that there is a unified scheme covering all realms of phenomena, and indeed, in the last analysis, the affairs of mankind as a particular case. The idealistic philosopher will not cease to search for such a scheme and it is right that he should do so. If and when he succeeds, however, it may well be that we shall find that the scheme which he has found is of very little practical use.

As a matter of fact, a very general scheme covering as particular cases a wide range of phenomena dare not, in the nature of things, be very specific about any one of the phenomena. It can only be specific about things which are common to all the phenomena; and of these there may be very few. The very general theory will be like a very evasive politician. As an active member of a group devoted to economy in public affairs, you come to him and ask what he has to say about expenditure on armaments, hoping perhaps to get a detailed budget, stating how much may be assigned to this and to that, and how much may be saved from armaments for peaceful projects. However, the reply you get is something like this: "Our expenditures should be such as to maintain a stable and safe economy which reflects security in all that pertains to our lives." Well, you don't get very much out of that; and as you leave, and as you pass through the door, there comes a man fanatically devoted to military preparedness who wishes to ask about budgets designed to secure the most up-to-date equipment for all that pertains to war; and on posing his question, the politician again replies: "Our expenditures should be such as to maintain a stable and safe economy which reflects security in all that pertains to our lives."

Any statement which has to cover a wide range of circumstances cannot, in the nature of things, say much which applies to all; and indeed, when the range of circumstances is infinitely wide, the safest thing is to say nothing. However, if you are expert in the art of oratory, you will be able to say it with force and conviction.

Let us consider, as an example, a physicist who studies the science of electrodynamics and gravitation separately and later desires to mould them into a common theory. It will be unnecessary for us to think of gravitation in the light of the general theory of relativity. The old Newtonian concept will suffice.

Our physicist studies the laws of the heavens and finds that they conform very well to the Newtonian law of gravitation. I point out to him that some of the celestial bodies are magnets and that their attractions for one another will be modified in form and degree by this circumstance. The physicist replies quite correctly that the phenomenon is of very small numerical magnitude and that he proposes to neglect it. Next day, I find the physicist in his laboratory studying the attraction

of magnets and of electrically charged bodies for one another. I point out that these bodies also attract gravitationally, and that he should take this into account. Again he replies, quite correctly, that in these experiments the gravitational effects are so small compared with the electromagnetic effects that he is justified in neglecting them. In other words, in one problem of the universe, our physicist neglects the phenomena which are the whole source of interest in another problem, in which other problem, moreover, the phenomena dominant in the first problem are now negligible.

Now neglect of the small gravitational effects in the electromagnetic experiments is justifiable so long as one maintains the principle that the gravitational effect *is*, in actuality, there. If the gravitational effect is *omitted*, even in the formulation of the general principles of the subject, on the basis of its being too small to detect in electromagnetic experiments, and if the laws of these experiments are, therefore, placed on the statute books without it, they will possess no power to recognize it in any other phenomena of nature where the circumstances may be different. They will, in fact, be in danger of actually denying its existence in any field whatever, and of rendering its subsequent discovery in the astronomical field a phenomenon puzzling to comprehend, and apparently antagonistic to the science of electrodynamics.

Now, if a general theory embracing electrodynamics and gravitation is provided, it may take care of problems in which gravitational forces and electrodynamical forces are equally important even though nature may present us with no such cases where they *are* of equal importance. Such a general theory is able to extrapolate itself to one end to a case where gravitation is unimportant and electrodynamics is all important and to extrapolate itself also to the other end to a case where the relative importance of these respective phenomena is reversed. The theory thus provides a bridge by means of which the two extreme cases are seen to be not inharmonious, whereas individual theories for each case, formulated on the basis of all that experiment can reveal, would appear at first sight mutually antagonistic.

However, the language of the bridge which spans electrodynamics and astronomy may not be very simple when asked to speak the story of either of these subjects separately. If I concentrate on the domain which is astronomy, I shall tend to paint pictures and make models characteristic of that end of the bridge, pictures which emphasize very strongly to my intuition the salient phenomena of astronomy, but pictures which would have to become more and more out of focus as I walked across the bridge to the realm of electrodynamics. And when I reached this realm, I would find them completely out of focus and unable to convey to me any meaning at all. On the other hand, if I start at the end of the bridge which is electrodynamics, and do the same kind of thing, I shall paint pictures and make models appropriate to

the most important phenomena characteristic of that end. And these pictures will, in turn, become more and more hazy as I cross the bridge to the end concerned with astronomy. If I am a philosopher, and willing to realize the limitations of my pictures at both ends of the bridge, I shall not be disturbed by their becoming hazy as I cross from one end to the other. However, if I am a non-philosophical astronomer, the pictures which I have painted, and the models which I have created to understand my subject will be very fundamental to me; and if I tamper with them my mind will protest that what I am doing produces nonsense. A similar thing will happen for the non-philosophic student of electrodynamics at the other end of the bridge. He will create his pictures and the elements of his creation will be for him the basis of reasonable understanding. Thus while the philosopher will be able to cross the bridge in contentment in either direction, adjusting himself to the scenery on the way, the non-philosophic astronomer and the non-philsophic student of electrodynamics will feel that their realms are quite distinct and that the laws of one subject have no connection with those of the other. While the general formulation of the philosopher will extrapolate harmoniously, both ways, from one end of the bridge to the other, the more specific pictures and models appropriate to the two ends will not extrapolate, for the elements of these pictures and models which are prominent in the phenomena at one end may be of negligible importance to the phenomena at the other end.

And yet, at each end of the bridge, the philosophically imperfect pictures appropriate to that end may be more useful than the generalized picture painted by the philosopher. Thus, three hundred years ago, many believed that light was composed of rays which traveled like arrows from source to image. Later came the wave picture of light, and later still the picture characteristic of the quantum theory. However, even today, the optician who insisted on making spectacles with philosophic regard to all the features of the quantum theory would soon go out of business. No, the optician makes his spectacles with no thoughts in mind other than those of his forerunners who thought entirely in terms of rays for all optical phenomena three hundred years ago.

It becomes increasingly important that the physicist, who has attained success in much of his specialized field by invoking certain principles, should not fail to inquire as to the extent to which those principles may play a leading role in other domains in which, perhaps, as yet they have not been utilized.

The Role of Pattern

The atomic physicist has been brought up to think in terms of particles with what he calls forces between them. He learned to do this in the early birth of astronomy and has clung to the procedure. In the

beginning the procedure was to seek the laws of motion of the particles. Thus, if, in astronomy, one gave the positions and velocities of the heavenly bodies at some instant, the laws were such as to spell out step by step how each of them moved; and in terms of those positions and velocities originally assigned, tell the inevitable story of what happens subsequently. In terms of positions and velocities taken as starting points, the system was one of predetermination. All motions calculable in this way, and consistent with some initially assigned sets of positions and velocities, were regarded as possible. Of course, different starting points resulted in very different developments. In some cases we should realize a body like a sun with other bodies traveling in circles or ellipses around it. In other cases, we should have bodies coming in from outer space, visiting the sun for a brief period, and returning to infinity by other paths. In still other cases, bodies would interweave their ways in complicated paths among their fellows. There were, in fact, innumerable *patterns* which could evolve from different starting points, and each of these patterns had its own peculiarity inherent in its own particular starting point. Thus, all sorts of different astronomical universes were possible insofar as the motions of the bodies which constituted them were concerned. However, the custom was not to concern oneself too much with the patterns as fundamental, but rather to regard them merely as the consequences of the particular positions and velocities which, by chance, had been originally specified.

When science came to regard atoms as groups of particles, the same kind of procedures was envisaged; although the laws of motion of the particles were spelled out in different fashions when other things like electromagnetic radiations became involved and claimed a place for the harmonization of such things as light, X-rays, wireless radiations, and so forth.

And as science advanced, particularly in the atomic realm, it became evident that the theoretical procedures born of astronomy were becoming increasingly unsuccessful in providing a description of all that happened. It became evident that the procedure which was likely to work was a procedure in which one concentrated more on inquiring as to the *patterns* which can exist in nature, patterns of motions of particles, in the first instance, and later, patterns of more abstract things which the physicist called "psi functions." The old *laws of motion* of particles formerly occupied the central stage of our interest. *Any* pattern to which they led could be regarded as permissible; and of the permissible patterns, there was an infinite variety. In the new era of atomic philosophy what remained of the old laws of motions of things was relegated to the service of limiting the patterns which could occur. The fundamental duty of these old laws, as dressed in their new garb, was to declare as meaningless all patterns but a limited set, the set which could be evolved out of them. The fundamental bricks of

nature's structure were patterns which, born of these laws, dictated the things which could occur as distinct from those which could not occur.

The bricks of nature—the atoms, the molecules—were, in principle, more like a set of oriental rugs than minute astronomical systems. These rugs, indeed, were only symbolic and with them there went a scheme of interpretation of their significance. There is a faint analogy between these rugs and the oriental rugs which adorn your houses, for I believe it is a fact that the various patterns and sub-patterns in these rugs are created with interpretable meanings.

And so, in science, what had formerly been laws of motions of particles were transformed to laws which determined what patterns could exist in nature, and with this scheme of things there went a key for the interpretation of the patterns. It was indeed, a far cry from one who thought to understand these laws of patterns in the sense in which, perhaps, he may have thought he understood the laws of astronomy. If you ask a maker of oriental rugs in what sense he *understands* the meaning of the rugs, he may rightly reply: "I do not have the problem of understanding why these rugs exist. I and my forerunners created the designs ourselves, but we have endeavored to weave into them a symbolic meaning which reflects the relationships of things in the world around us. Why these things should be and why that which happens does happen, we know not. Our function is that of systematic cataloguers of events, and our rugs are the symbolic catalogs."

Now, I do not mean to say that the citizens of Arabia who make rugs would say everything that I have put into their mouths if I started with them a discussion on the matter. All I maintain is that they might have said it; and if, in the saying of it, they had sought to develop a systematic scheme symbolizing the ways of the atomic world, instead of the limited domain of living things and the immediate elements of their experiences, hope, love, fear, and so forth, they might have been on the way to doing something rather similar to what the atomic physicist is doing today. Of course, the atomic physicist has at his disposal that great logical scheme of mathematics which he can use freely in his designs, and he is not limited to the utilization of simple elements of geometry.

I have spoken of the patterns of atomic structure as being abstract things distinct from pictures of particles and other material things which may be around. However, the patterns appropriate to groups of atoms which constitute the things we see, come, in some of their manifestations, to assume actual *shapes* of things in the elementary meaning of that word. And so, today, we see patterns, born in the understanding of atoms, extending themselves into combinations of atoms—to molecules. Here the pattern is as yet unobserveable to the eye, and its abstract form must be inferred from chemical behavior. But from molecules, pattern extends itself into large structures, into

crystals where form is evident in that which can be perceived by the eye. And in this domain of crystals, pattern provides a rich harvest of phenomena which, in the role known as that of semi-conductors, has, within the last two decades, revolutionized the world of electronics, and here man found that his colossal achievement in inventing the radio tube and all that goes with it, was already anticipated and beaten by nature in providing what we now call transistors, which reduce in size and increase in compactness all such electronic devices.

Pattern has always been evident on a large scale in biological structures, but now we find it playing a fundamental role, not only in things which can readily be seen, but in the seeds of life itself, in the chromosomes of the cells whose behavior is so vital in cell division, and in the transmission of hereditary characteristics. And through such processes, we may, in time, learn to comprehend that crowning achievement of pattern to be found in man himself, an achievement in which a single germ cell contains in itself a pattern which insures that the being to which it grows shall duplicate, not only the general form, but many of the characteristics of his ancestors. The substance of the individual dies several times during what we call the span of his life, but pattern goes on from generation to generation; and even an abnormality in the being, a crooked finger, or a prominent jaw formation, can survive in its pattern for a thousand years. One thing about man, even as he is evident to those around him, goes far towards being immortal. It is pattern.

Relation Between the Sciences of Yesterday and Today

To return to the title of this address, the science of yesterday was largely the evolution of blind discovery of phenomena, the discovery of fire, the discovery of the potentialities of the wheel, the ingenious combinations of circumstances and principles, which were ever before us, to the use of man. There was another crude but nevertheless practical and extremely ingenious ordering of the things of nature in the systems of laws formulated by Newton and Galileo, and later by the giants of science of the nineteenth century, culminating in a sensing of the potentialities of the newly discovered phenomena of electricity. These developments led to the formulation of the general principles of electrical engineering, the realization of the dynamo, the electric motor, and later wireless telegraphy and so forth. Then, starting towards the end of the nineteenth century, came an era of new interests. Experimental researches resulted in the discovery of phenomena not continually evident to the eye of man, phenomena which could only be brought into existence by the efforts of his researchers. The behavior of the planets, the general phenomena which govern mechanical machines, were always displayed before mankind and awaited only the exercise

of man's ingenuity to harmonize them, and use them to his service when possible. The phenomena of electrical engineering of three quarters of a century ago were not things evident to the eye until researches ferreted them out and organized them into purposeful activity. The later developments beginning towards the end of the nineteenth century concerned the contents of the boxes of which I have spoken earlier. They concerned the discovery of the electron, the proton, X-rays, and allied phenomena of atomic behavior. These things were completely unevident to the eye of man until research forced them out of hiding and caused them to reveal their activities in newly created devices which would not have existed except for man's activity. Having become released from their bondage of obscurity, it became clear that these strange new things had, all along, been playing a part in phenomena which had been available to man's viewing from time immemorial. Up to this time, the laws of chemistry were largely empirical as were the laws of biology. The laws of what we call Physical astronomy, as distinct from those of celestial mechanics, were in a very scrappy state as regards consistency of understanding, and the things of greatest interest had not forced themselves spontaneously upon man's notice. However, the development of the great telescopes and allied equipment presented an entirely new challenge for the understanding of things and behaviors vastly different in both scale and nature from those which, up to that time, had been the only things displayed for man's curiosity. And it came to pass that the new discoveries in connection with atomic laws went far towards providing for these things an understanding which would have been impossible without them. These same discoveries of the atomic realm did much to provide a more complete picture of what was going on in chemistry, and even in biology.

THE SCIENCE OF TOMORROW

And here we stand today. We have a conciousness of vast accomplishment in the interplay of what we call fundamental experimental research and fundamental theoretical research. Much harmony has been brought into things which would otherwise be obscure; and yet, the returns of the harvest of discovery have tended to reveal so much more to be fitted into the scheme and have given evidence of so much more yet to be discovered, that the expected labors of the future may well outweigh all those of the past. And what direction may these labors be expected to take in this era of the future—in this science of tomorrow?

While there is much yet to be done in correlating and enriching all that is known about what we call the material world, I feel that before long, we shall have to face the problem of the nature of life and of all that goes with it, if real progress is to be made. We cannot, for ever, keep the laws of dead matter separated from those of living things; for

after all, everything that happens as the result of our efforts in the utilization of what we have already learned must be initiated by the mind of man. I can imagine the heavens to go on their courses without any attention from mankind. I can be happy in the thought of a continual process of activity which, in its gross aspects at any rate, follows the kind of deterministic behavior which, a hundred years ago, might have been thought to be the "way of life" of all nature. But if, today, I make an atomic bomb which does drastic things, it is I who formed the decision to make it; and in so doing, I interfere with what would have happened had I not made this decision. At this point, the mind of man seizes upon the otherwise smooth running of things, and, in some way, that which is in my mind interlocks with inanimate nature to direct its course.

The Role of New Entities

And in facing the necessity of bringing harmony into realms which today stand apart, what has the experience of the past taught us? We have a clue in what has happened in the domain of atomic structure itself. There was a time when all we had to work with were atoms regarded as indivisible things, without any properties other than were provided by empiricism as demanded by the laws of chemistry. No progress was being made in understanding the laws of spectroscopy or the laws which related the elements to one another. Even the periodic table was an unfathomable mystery. Then came the discovery of the electron and the proton, two entities whose existence had not before been recognized, and at least a promise of further understanding was achieved. It was a faith in this promise which caused many to believe that the end of discovery was near. However, a barrier to further progress was soon reached. Many had wished to invoke the possibility of another kind of particle—a neutral particle—but conservative science, having with reluctance accepted two new things, the electron and the proton, smaller than the atom, looked with great distaste upon any upstart who wanted more atomic bricks to play with; and it was not until, through experiment, a neutral particle, the neutron, was proved to exist that progress went ahead with leaps and bounds.

We can readily understand the hesitancy of science to accept a neutral particle. One had almost come to regard as self-evident the principle that all atomic forces were electrical, and how could a neutral particle exert a force on anything or indeed, how could it be influenced by anything? In the spirit of the times it had to be regarded as a completely dead entity. Perhaps the greatest clash with convention was the recognition of the fact that this entity, dead in the sense of all understandable happenings, could indeed play a part in its own way, a way so foreign to anything which was in the conventional picture. It

was not so much by the fact that the neutron represented a new particle that science became disturbed, but rather that it represented a new set of relationships between things, a relationship which was not in the picture before. One had to admit what are called *nuclear forces* as distinct from electromagnetic forces—a new world of law and order. And what was more astonishing, one had to provide for interlocking relationships between this new domain of phenomena and the old domain which was so unlike it, and which, up to this time, had claimed authority over all nature.

Harmonization of the Science of Today and That of Tomorrow

And so, in contemplating the harmonization of life with what we call the laws of inanimate matter, I expect to find a new set of laws, laws which do not deny anything we had before except in the denial of the claim of those laws to finality. And I expect to find these new laws interweaving with the old knowledge in such a manner as to produce a more comprehensive whole, a whole in which all sense of barriers has become dissolved in an all-embracing harmony. For many purposes it may be convenient to keep the new domain separate from the old, as the maker of spectacles keeps his science of geometrical optics separated from the quantum theory of light; but there will be bridges connecting all parts of the new territory with the old domains in such fashion that he who travels across these bridges will have no sense of sudden change; and even as one who travels from tropical regions to the poles can accommodate himself to his satisfaction at each stage of the journey, so the philosopher, in traveling over this wider domain which I envisage, will find himself content wherever he may be.

In developing the foregoing thoughts I have called attention to the rapid advance which took place in physics itself once one was willing to accept a new particle, the neutron, and furnish it with the wherewithal to operate. Now I do not expect it to be necessary to find a new *particle* which will cement the old materialistic realm with the realm of life and all that goes with it, but I may expect to find the formal recognition of some kind of a new entity differing from those which we have encountered in physics. I do not necessarily expect that this entity will be something which can be described in terms of space and time, although I shall expect it to be accompanied by well-defined laws of operation which provide, not only for the activities peculiar to its own purposes, but for the possibility of cementing it logically with the knowledge of the past. We must not be too astonished at the invocation of an entity which does not call for expression in terms of space and time. After all, I may speak of such things as good and evil without accompanying them with coordinates x, y, z, t, to express where they are and when they were there. For the sophisticated physicist, I may recall that even the coordinates which represent Fourier amplitudes in the

analysis of radiation in an ideal box are not coordinates of a material point in ordinary space, but, as coordinates in an abstract, multi-dimensional space, they perform a useful service in physics. In the last analysis much that is spoken of in the quantum theory of physics involves concepts having little to do with the old conventional notion associated with the expression of all relevant concepts in terms of some thing or things having positions at certain times. I shall not be surprised to find the new entity playing a part in the survival of pattern, so dominant in living things. I hesitate to limit its potentialities by giving it a name already appropriated and endowed with properties of vagueness too foggy to be permitted in a scientific discussion, and so I will not call it by the name "soul." If it is to be of service, it must not shrink away from its duties and take refuge as part of high-sounding sentences. Its functions and modes of operation must be well-defined and it is only natural that in conventional science it will have to go through the process of skeptic criticism which has fallen to the lot of all of its predecessors in the materialistic realm. I should expect to find it play a role in those phenomena which for long have lain in the borderland between what is accepted by all and what is accepted only by few, even though representatives of the few may be found in all periods of man's history. I refer to such things as extrasensory perception, the significance of the immortality of man, clairvoyance, and allied phenomena, and the significance of the fact that our universe exhibits what we may call a planned design, whether or not we are willing to admit the hazy notion of a planner, or say what we mean by that postulate.

Predetermination and a Planned Universe

Perhaps the existence of the universe as an entity with strongly planned features provides the greatest argument against use of the undeniable fact that if we are willing to work hard enough and involve ourselves in a sufficient complexity in mathematical expression, we can possibly regard any universe as operating on a principle of predetermination. In general, the principle invoked in such an arbitrary manner, may rule out many notions which seem so important in the life of mankind, by regarding everything as inevitable, even as in the parable of the slave and his master at the beginning of this lecture the theft by the slave was inevitable and the beating received for it was also inevitable. If the universe were a chaotic affair without any of the properties which I have associated with the word planned, there might be some sense in falling back on predetermination, but to invoke such a principle with things as they are is something like to assert that a cathedral of great beauty, which I had not seen before, was formed by the accumulation of dust in an accidental manner through the ages.

New Domains for Scientific Investigation

In discussing such matters as I am now venturing near, I think it is essential to avoid all theological doctrine as a starting point. I would rather see a theological doctrine emerge spontaneously as part of the over-all scheme of nature, than I would see the workings of nature forced into a frame provided by a preconceived theological doctrine as a starting point.

In the past it has been a tradition of mankind to divide phenomena into two classes, those which may be investigated, and those concerning which we should not inquire. Between these two sets of phenomena there has been a barrier, and to cross that barrier was a sin against dogma or, in less solemn vein, a violation of sound principles of research only to be undertaken by those who are a little queer. As times progressed, this barrier has shifted, so that all astronomy now lies on the respectable side of it, in spite of the fact that three hundred years ago much of it lay in the forbidden region, where also much of the embryo science of chemistry was to be found. Today, chemistry is thoroughly established in the unrestricted region.

Even as many radicals become conservatives when they rise to power, so the science of the materialistic age, much of which lay on the dark side of the barrier in the past, on becoming promoted to the free side, started to fortify still further the barrier which it had passed, so that things which did not readily find a place in its philosophy were held in the forbidden region. Yet behind this fortification of division which materialistic science itself has strengthened, stand the shadows of bygone days: the philosophies, the practices, the beliefs, and religions of ancient times, so vulnerable in many of the dogmas with which history had endowed them, that they oft fell an easy prey to the shafts of the newborn science of our era. The weaknesses in their armor bred a kind of conviction that all the wisdom of the ancient past was afflicted with the disease of superstition, a disease eating like a cancer into its whole system. Thus, many things which had been accepted for thousands of years were cast into the category of witchcraft. In the totality of these things there were, however, certain realms which, in virtue of the power which had supported them through the ages and because of their moral influence on mankind, stood with some security against the attacks of modern philosophy. These were, for the most part, the standard religions of mankind. There was a sort of truce between the two camps, a truce in which the realm of religion ruled on Sundays, while the materialistic philosophy governed the rest of the week. Some things, well accepted in the past, but apparently at variance with materialism, found themselves without the powerful support accorded to the great religions and so they were left to the ridicule of the new age. Some of these things which had been part of the doctrine of the churches of the bygone era found themselves disdained by the faiths which had nurtured

them, and the guardians of the faiths became anxious to avoid contamination with practices which might be attacked with some apparent success by the warriors of the new age. Thus, healing by the laying on of hands, belief in the existence of spirit entities in our midst, even such were cast out by the religions which had originally fostered them, or if admitted at all, were retained as machinations of the devil, a being so beloved by the faiths that have created him that he has succeeded in holding his own in religion in the face of science itself. Naturally, at times he became very convenient as an agent to whom one could attribute all the shortcomings and inconsistencies in the faiths and dogmas which sought to rule, as well as his own shortcomings. In contemplating his identity, one is reminded of the little girl who, on being asked by her younger sister the question: "Is there really a devil?," replied: "No, of course not, it's just like Santa Claus; it's Daddy."

And now what we call orthodox science has itself grown a type of philosophy so different from the old science born of materialism that if it were forced to pause long enough to confess what, a hundred years ago, it would have called its philosophic sins, it would find those sins no more free from materialistic criticism than much of the sins of philosophy which it has held behind the barrier.

The Ultimate Harmonization of Science

Perhaps some day, not too far distant, orthodox science will find the urge to extend its domain of inquiry into regions formerly forbidden, and in the hope that all the phenomena of nature may find a place in one larger scheme of harmonization. I would hope that in this more comprehensive philosophy no man would have occasion to forsake any of the ideals which in the past he had fostered. When this condition arrives, I envisage a sage charged with the duty of answering the questions of all who would make inquiry. The musician will say: "Where is my art in this scheme?" and the sage will reply: "See, it is here, complete in itself, but joined by this bridge, in perfect logical continuity with yon domain which is the domain of abstract mathematics." And the priest will ask: "Where are the essentials of my faith in which I have lived and which has been my anchor of security?" And the sage will answer: "Cast your vision upon yon territory. There you will find it. It is joined by a bridge of great beauty to the domain of your arch-enemy, the domain which was formerly that of materialistic science."

And in this picture those things for which the mind and soul long shall no longer appear veiled in nebulous shrouds of uncertainty, but shall stand out as jewels adorning the greater universe in all its richness and splendor. And if some doubtful inquisitor should ask of the sage: "Where, in all this, shall I find the devil who has meant so much to me in my life?" he will receive the reply: "The devil—oh, the devil! He is in hell. You will find hell behind the old barrier, and the devil is the only occupant."

RELATIONS CONCERNING REFRACTING SURFACES, WAVEFRONTS, AND PHASE ERRORS

BY

DAVID K. CHENG[1]

ABSTRACT

Using vector notation of differential geometry, general relationships connecting the incident wavefront, two refracting surfaces, and the refracted wavefront have been analyzed. Formulas have been derived for determining the equation of the fourth when the expressions of the other three surfaces are given. These formulas are useful for the determination of aperture phase errors in a defocused lens and for microwave lens design.

1. INTRODUCTION

There exist in the literature (1)[2] general formulas for determining the phase error in an aperture plane of any given reflector when the expression for the incident wavefront is known. The determination of aperture phase error is important because it has a direct bearing on the radiation pattern of a microwave antenna system (2). Three surfaces are involved in a reflector problem, namely, the incident wavefront, the reflecting surface, and the reflected wavefront. A problem of greater complexity is one that involves four surfaces. Examples in microwave antenna systems that belong to this category are those which employ dielectric lenses and mirror-reflector arrangements.

This paper analyzes the interrelationship among the incident wavefront, the two surfaces of a homogeneous dielectric lens, and the refracted wavefront. General formulas have been obtained for determining the equation of the fourth when the expressions of the other three surfaces are specified. Aperture phase distributions of a defocused dielectric lens are also determined and compared with those for a paraboloidal reflector with on-axis defocusing.

2. GENERAL LENS RELATIONSHIPS

In Fig. 1 is shown a vectorial representation in the xz-plane of the surfaces and wavefronts for a homogeneous dielectric lens of relative permittivity η. In the notations of differential geometry the following vectors may be identified:

$\bar{A}$ = source vector = $a\bar{k}$
$\bar{R}_1$ = vector representing the first surface of the lens

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[2] The boldface numbers in parentheses refer to the references appended to this paper.

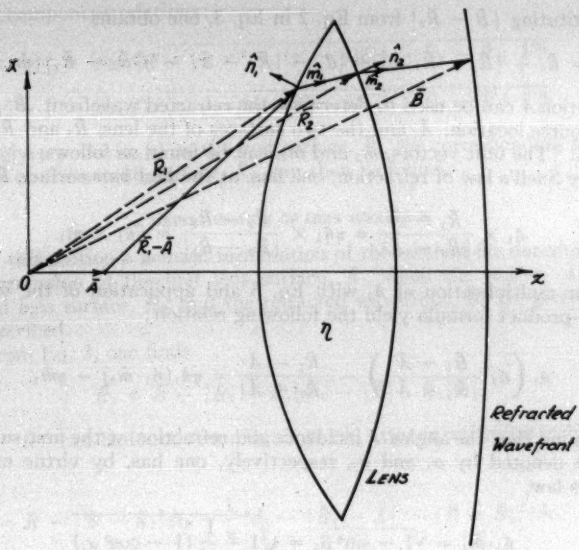

FIG. 1. Vectorial representation of surfaces and wavefronts for a homogeneous dielectric lens.

$\bar{R}_2$ = vector representing the second surface of the lens
$\bar{B}$ = vector representing the refracted wavefront

$\hat{n}_1$ and $\hat{n}_2$ are unit normals to the first and second surfaces of the lens, and $\hat{m}_1$ and $\hat{m}_2$ are unit vectors along the refracted rays emerging from the first and second surfaces, respectively.

Let d_g and d_o be the geometrical and optical path lengths, respectively, between the source and the refracted wavefront. Then

$$d_g = B(0,0) - a \tag{1}$$

and

$$d_o = |\bar{R}_1 - \bar{A}| + \eta|\bar{R}_2 - \bar{R}_1| + |\bar{B} - \bar{R}_2|. \tag{2}$$

Also, from Fig. 1, the vector representing the refracted wavefront can be written as

$$\bar{B} = \bar{R}_1 + |\bar{R}_2 - \bar{R}_1|\hat{m}_1 + |\bar{B} - \bar{R}_2|\hat{m}_2. \tag{3}$$

Substituting $|\bar{B} - \bar{R}_2|$ from Eq. 2 in Eq. 3, one obtains

$$\bar{B} = \bar{R}_1 + |\bar{R}_2 - \bar{R}_1|\hat{m}_1 + \{d_o - |\bar{R}_1 - \bar{A}| - \eta|\bar{R}_2 - \bar{R}_1|\}\hat{m}_2. \quad (4)$$

Equation 4 can be used to determine the refracted wavefront, $\bar{B}$, when the source location, $\bar{A}$, and the two surfaces of the lens, $\bar{R}_1$ and $\bar{R}_2$, are given. The unit vectors, $\hat{m}_1$ and $\hat{m}_2$, can be found as follows.

By Snell's law of refraction, one has, at the first lens surface $\bar{R}_1$,

$$\hat{n}_1 \times \frac{\bar{R}_1 - \bar{A}}{|\bar{R}_1 - \bar{A}|} = \eta\hat{n}_1 \times \frac{\bar{R}_2 - \bar{R}_1}{|\bar{R}_2 - \bar{R}_1|} = \eta\hat{n}_1 \times \hat{m}_1. \quad (5)$$

Vector multiplication of $\hat{n}_1$ with Eq. 5 and application of the vector triple-product formula yield the following relation:

$$\hat{n}_1\left(\hat{n}_1 \cdot \frac{\bar{R}_1 - \bar{A}}{|\bar{R}_1 - \bar{A}|}\right) - \frac{\bar{R}_1 - \bar{A}}{|\bar{R}_1 - \bar{A}|} = \eta\hat{n}_1(\hat{n}_1 \cdot \hat{m}_1) - \eta\hat{m}_1. \quad (6)$$

Assuming that the angles of incidence and refraction at the first surface $\bar{R}_1$ be denoted by α_1 and β_1, respectively, one has, by virtue of the Snell's law,

$$\hat{n}_1 \cdot \hat{m}_1 = \sqrt{1 - \sin^2\beta_1} = \sqrt{1 - \frac{1}{\eta^2}(1 - \cos^2\alpha_1)}$$

$$= \sqrt{1 - \frac{1}{\eta^2}\left[1 - \left(\hat{n}_1 \cdot \frac{\bar{R}_1 - \bar{A}}{|\bar{R}_1 - \bar{A}|}\right)^2\right]}. \quad (7)$$

Substitution of Eq. 7 in Eq. 6 gives

$$\hat{m}_1 = \frac{1}{\eta}\left\{\frac{\bar{R}_1 - \bar{A}}{|\bar{R}_1 - \bar{A}|} - \hat{n}_1\left[\hat{n}_1 \cdot \frac{\bar{R}_1 - \bar{A}}{|\bar{R}_1 - \bar{A}|} - \sqrt{\eta^2 - 1 + \left(\hat{n} \cdot \frac{\bar{R}_1 - \bar{A}}{|\bar{R}_1 - \bar{A}|}\right)^2}\right]\right\}. \quad (8)$$

The unit vector $\hat{m}_2$ can be determined by a similar procedure through application of Snell's law of refraction at the second lens surface $\bar{R}_2$:

$$\hat{m}_2 = \eta\hat{m}_1 - \hat{n}_2\{\eta(\hat{n}_2 \cdot \hat{m}_1) - \sqrt{1 - \eta^2[1 - (\hat{n}_2 \cdot \hat{m}_1)^2]}\}. \quad (9)$$

It is now necessary to find the corresponding points on the two surfaces of the lens along a ray trajectory. Since the unit vector $\hat{m}_1$ determines the direction of the ray within the lens, one can write

$$\bar{R}_2(x', y') - \bar{R}_1(x, y) = |\bar{R}_2(x', y') - \bar{R}_1(x, y)|\hat{m}_1. \quad (10)$$

Coordinates x' and y' can be determined from Eq. 10 through the fol-

lowing component relations:

$$x' - x = (\hat{m}_1 \cdot \hat{\imath})\sqrt{(x' - x)^2 + (y' - y)^2 + [(\bar{R}_2 - \bar{R}_1) \cdot \hat{k}]^2} \quad (11)$$

$$y' - y = (\hat{m}_1 \cdot \hat{\jmath})\sqrt{(x' - x)^2 + (y' - y)^2 + [(\bar{R}_2 - \bar{R}_1) \cdot \hat{k}]^2}. \quad (12)$$

For a given point (x, y) on the first surface, $\bar{R}_1$, the corresponding point (x', y') on the second surface, $\bar{R}_2$, can then be found.

3. DETERMINATION OF LENS SURFACE R_1

In this section a general formulation of the method for determining the expression of the first lens surface, $\bar{R}_1$, when the source, $\bar{A}$, the second lens surface, $\bar{R}_2$, and the refracted wavefront, $\bar{B}$, are given will be described.

From Eq. 3, one finds

$$\bar{R}_1 = \bar{B} - |\bar{R}_2 - \bar{R}_1|\hat{m}_1 - |\bar{B} - \bar{R}_2|\hat{m}_2. \quad (13)$$

$|\bar{R}_2 - \bar{R}_1|$ can be found from Eq. 2 which, after substitution in Eq. 13 gives

$$\bar{R}_1 = \bar{B} - |\bar{B} - \bar{R}_2|\hat{m}_2 - \frac{1}{\eta}\{d_0 - |\bar{R}_1 - \bar{A}| - |\bar{B} - \bar{R}_2|\}\hat{m}_1. \quad (14)$$

In Eq. 14, η, d_o, $\bar{A}$, $\bar{B}$, and $\bar{R}_2$ are given; $\hat{m}_2$ can be found by standard formulas because it is a unit normal to $\bar{B}$; the quantities $\hat{m}_1$ and $|\bar{R}_1 - \bar{A}|$ have yet to be determined.

$\hat{m}_1$ can be found by starting from the relation at the second lens surface

$$\hat{n}_2 \times \hat{m}_1 = \frac{1}{\eta}\hat{n}_2 \times \hat{m}_2 \quad (15)$$

and following the same procedure that led to $\hat{m}_2$ in Eq. 9.

$$\hat{m}_1 = \frac{1}{\eta}\{\hat{m}_2 - \hat{n}_2[(\hat{n}_2 \cdot \hat{m}_2) - \sqrt{\eta^2 - 1 + (\hat{n}_2 \cdot \hat{m}_2)^2}]\}. \quad (16)$$

In order to determine $|\bar{R}_1 - \bar{A}|$, re-write Eq. 14 as follows:

$$\bar{R}_1 - \bar{A} = (\bar{R}_2 - \bar{A}) - \frac{1}{\eta}(d_o - |\bar{B} - \bar{R}_2|)\hat{m}_1 + \frac{1}{\eta}|\bar{R}_1 - \bar{A}|\hat{m}_1. \quad (17)$$

Taking the dot product of the quantities with themselves on both sides of Eq. 17 and re-arranging terms, one obtains a quadratic equation in $|\bar{R}_1 - \bar{A}|$.

$$\frac{1}{\eta^2}(\eta^2-1)|\bar{R}_1-\bar{A}|^2 - \frac{2}{\eta}\left\{(\bar{R}_2-\bar{A})\cdot\hat{m}_1-\frac{1}{\eta}(d_o-|\bar{B}-\bar{R}_2|)\right\}|\bar{R}_1-\bar{A}| - \left|\bar{R}_2-\bar{A}-\frac{1}{\eta}(d_o-|\bar{B}-\bar{R}_2|)\hat{m}_1\right|^2=0. \quad (18)$$

The solution for $|\bar{R}_1-\bar{A}|$ is

$$|\bar{R}_1-\bar{A}|=\frac{\eta}{\eta^2-1}\left\{(\bar{R}_2-\bar{A})\cdot\hat{m}_1-\frac{1}{\eta}(d_o-|\bar{B}-\bar{R}_2|)\right\} \times\left[1+\sqrt{1+(\eta^2-1)\frac{\left|\bar{R}_2-\bar{A}-\frac{1}{\eta}(d_o-|\bar{B}-\bar{R}_2|)\hat{m}_1\right|^2}{\left\{(\bar{R}_2-\bar{A})\cdot\hat{m}_1-\frac{1}{\eta}(d_o-|\bar{B}-\bar{R}_2|)\right\}^2}}\right]. \quad (19)$$

The solution with a negative sign before the radical in Eq. 19 is not useful because the radical has a value greater than unity and the quantity inside the curved brackets is positive. Substitution of Eqs. 16 and 19 in Eq. 14 will determine $\bar{R}_1$ in terms of $\bar{A}$, $\bar{R}_2$, $\bar{B}$ and η. It is to be noted that the above formulation appears to be quite complex because it is a general formulation. For simple problems (for example, when both $\bar{R}_2$ and $\bar{B}$ are plane surfaces) it is easier to work directly from the fundamental geometrical relationships than to use the most general formulas.

4. DETERMINATION OF LENS SURFACE R_2

Parallel to the development in the previous section, the expression for the second lens surface, $\bar{R}_2$, when the source, $\bar{A}$, the first lens surface, $\bar{R}_1$, and the refracted wavefront, $\bar{B}$, are given will be described in this section. The general formulation follows closely that for the previous case.

$$\bar{R}_2 = \bar{R}_1+|\bar{R}_2-\bar{R}_1|\hat{m}_1 = \bar{R}_1+\frac{1}{\eta}\{d_o-|\bar{R}_1-\bar{A}|-|\bar{B}-\bar{R}_2|\}\hat{m}_1. \quad (20)$$

The unit vector $\hat{m}_1$ can be found readily from $\bar{A}$, $\bar{R}_1$, and $\hat{n}_1$ by Eq. 8. To determine $|\bar{B}-\bar{R}_2|$, re-write Eq. 20 as

$$\bar{B}-\bar{R}_2=(\bar{B}-\bar{R}_1)-\frac{1}{\eta}(d_o-|\bar{R}_1-\bar{A}|)\hat{m}_1+\frac{1}{\eta}|\bar{B}-\bar{R}_2|\hat{m}_1. \quad (21)$$

It is seen that Eq. 21 is exactly similar to Eq. 17 with the following pairs of quantities interchanged:

$$(\bar{B} - \bar{R}_2) \leftrightarrow (\bar{R}_1 - \bar{A})$$
$$(\bar{B} - \bar{R}_1) \leftrightarrow (\bar{R}_2 - \bar{A})$$

which are equivalent to

$$\bar{B} \leftrightarrow -\bar{A}$$
$$\bar{R}_2 \leftrightarrow -\bar{R}_1.$$

The expression for $|\bar{B} - \bar{R}_2|$ can therefore be written down directly from Eq. 19.

$$|\bar{B}-\bar{R}_2| = \frac{\eta}{\eta^2-1}\left\{(\bar{B}-\bar{R}_1)\cdot\hat{m}_1 - \frac{1}{\eta}(d_o - |\bar{R}_1-\bar{A}|)\right\}$$
$$\times\left[1+\sqrt{1+(\eta^2-1)\frac{\left|\bar{B}-\bar{R}_1-\frac{1}{\eta}(d_o-|\bar{R}_1-\bar{A}|)\hat{m}_1\right|^2}{\left\{(\bar{B}-\bar{R}_1)\cdot\hat{m}_1-\frac{1}{\eta}(d_o-|\bar{R}_1-\bar{A}|)\right\}^2}}\right]. \quad (22)$$

Substitution of Eqs. 8 and 22 in Eq. 20 will determine $\bar{R}_2$ in terms of $\bar{A}$, $\bar{R}_1$, $\bar{B}$ and η.

$\bar{R}_2$ can be alternatively determined in another manner as follows.

$$\bar{R}_2 = \bar{B} - |\bar{B} - \bar{R}_2|\hat{m}_2 \quad (23)$$

$$\bar{R}_2 = \bar{R}_1 + |\bar{R}_2 - \bar{R}_1|\hat{m}_1. \quad (24)$$

Subtracting and taking dot products with $\hat{m}_2$, one has

$$|\bar{B} - \bar{R}_2| = (\bar{B} - \bar{R}_1)\cdot\hat{m}_2 - |\bar{R}_2 - \bar{R}_1|\hat{m}_1\cdot\hat{m}_2. \quad (25)$$

From Eq. 2, one has

$$|\bar{B} - \bar{R}_2| = d_o - |\bar{R}_1 - \bar{A}| - \eta|\bar{R}_2 - \bar{R}_1|. \quad (26)$$

Equating the right-hand sides of Eqs. 25 and 26, one finds

$$|\bar{R}_2 - \bar{R}_1| = \frac{d_o - |\bar{R}_1 - \bar{A}| - (\bar{B} - \bar{R}_1)\cdot\hat{m}_2}{\eta - \hat{m}_1\cdot\hat{m}_2}. \quad (27)$$

$\bar{R}_2$ can then be obtained by substituting Eq. 27 in Eq. 24

$$\bar{R}_2 = \bar{R}_1 + \left\{ \frac{d_o - |\bar{R}_1 - \bar{A}| - (\bar{B} - \bar{R}_1)\cdot m_2}{\eta - \hat{m}_1 \cdot \hat{m}_2} \right\} \hat{m}_1 \qquad (28)$$

where $\hat{m}_2$ is a unit normal to $\bar{B}$ and $\hat{m}_1$ can be determined from Eq. 8.

The coordinate relationships between surfaces $\bar{R}_1$ and $\bar{B}$ can be found by noting that

$$(\bar{B} - \bar{R}_2)\cdot \frac{\partial \bar{B}}{\partial x'} = 0 \qquad (29a)$$

$$(\bar{B} - \bar{R}_2)\cdot \frac{\partial \bar{B}}{\partial y'} = 0. \qquad (29b)$$

By solving Eqs. 28 and 29 simultaneously, it is possible to express the coordinates on $\bar{B}$ in terms of the corresponding ones on $\bar{R}_1$, so that

$$x' = h_1(x, y) \qquad (30a)$$

$$y' = h_2(x, y). \qquad (30b)$$

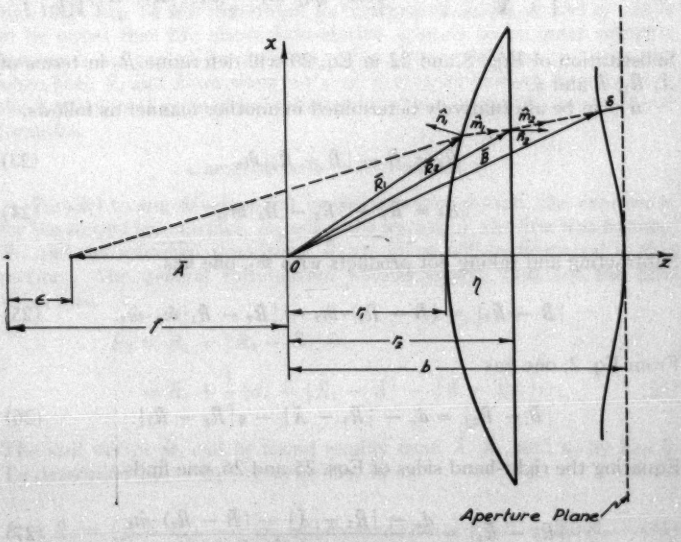

FIG. 2. A defocused dielectric lens.

5. APERTURE PHASE CHARACTERISTICS OF A DEFOCUSED LENS

In order to be more specific suppose it is desired to determine the phase characteristics in an aperture plane of a hyperboloidal dielectric lens when a point source is displaced from the focal position. Refer now to Fig. 2. The problem is as follows:

Given: $\bar{A} = -(f - \epsilon)\hat{k}$, ϵ = defocus distance

$\bar{R}_1 = x\hat{i} + y\hat{j} + z(x, y)\hat{k}$ (hyperboloidal surface)

where

$$z = r_1\sqrt{1 + \frac{x^2 + y^2}{f^2 - r_1^2}}, \quad f = \eta r_1$$

$$\bar{R}_2 = x'\hat{i} + y'\hat{j} + r_2\hat{k} \text{ (plane surface).}$$

To find: Phase Error $\delta(x, y)$.

Solution: The reference aperture plane is taken to be at a distance b from the origin o of the coordinate system. δ as shown in Fig. 2 for a typical dashed ray trajectory is actually the path-length difference (from the point source to the aperture plane) between the typical ray and the one along the z-axis. However, in order to avoid the necessity of introducing wavelength, δ will be referred to as phase error. As constructed, δ is a phase lead.

For the typical ray, the total path length from the point source to the aperture plane is composed of three portions:

$$\begin{aligned} &\text{Source to } \bar{R}_1\text{:} && |\bar{R}_1 - \bar{A}| \\ &\bar{R}_1 \text{ to } \bar{R}_2\text{:} && \eta|\bar{R}_2 - \bar{R}_1| \\ &\bar{R}_2 \text{ to } \bar{B}\text{:} && \frac{b - \bar{R}_2\cdot\hat{k}}{\hat{m}_2\cdot\hat{k}} \end{aligned}$$

Hence,

$$\delta = d_0 - \frac{b - \bar{R}_2\cdot\hat{k}}{\hat{m}_2\cdot\hat{k}} + \eta|\bar{R}_2 - \bar{R}_1| + |\bar{R}_1 - \bar{R}| \tag{31}$$

where

$$d_0 = (b - r_2) + \eta(r_2 - r_1) + (r_1 + f - \epsilon). \tag{32}$$

Since $\bar{R}_2$ is given as a plane surface normal to the lens axis, one has

$$\hat{n}_2 = \hat{k}.$$

Hence, from Eq. 9, one obtains

$$\hat{m}_2\cdot\hat{k} = \sqrt{1 - \eta^2[1 - (\hat{m}_1\cdot\hat{k})^2]}. \tag{33}$$

The quantity $(\hat{m}_1 \cdot \bar{k})$ under the radical can be obtained from Eq. 8.

$$\hat{m}_1 \cdot \bar{k} = \frac{1}{\eta}\left\{\frac{\bar{R}_1 - \bar{A}}{|\bar{R}_1 - \bar{A}|} \cdot \bar{k} - (\hat{n}_1 \cdot \bar{k}) \times \left[\hat{n}_1 \cdot \frac{\bar{R}_1 - \bar{A}}{|\bar{R}_1 - \bar{A}|} - \sqrt{\eta^2 - 1 + \left(\hat{n}_1 \cdot \frac{\bar{R}_1 - \bar{A}}{|\bar{R}_1 - \bar{A}|}\right)^2}\right]\right\}. \quad (34)$$

It is then necessary to determined $(\bar{R}_1 - \bar{A})/|\bar{R}_1 - \bar{A}|$ and $\hat{n}_1$ first.

$$\frac{\bar{R}_1 - \bar{A}}{|R_1 - A|} = \frac{x\bar{\imath} + y\bar{\jmath} + [z(x, y) + f - \epsilon]}{\sqrt{x^2 + y^2 - [z(x, y) + f - \epsilon]^2}}. \quad (35)$$

The formula for the unit normal to $\bar{R}_1$ is

$$\hat{n}_1 = \frac{\bar{R}_{1x} \times \bar{R}_{1y}}{|\bar{R}_{1x} \times \bar{R}_{1y}|} \quad (36)$$

with

$$\bar{R}_{1x} = \frac{\partial \bar{R}_1}{\partial x} = \bar{\imath} + z_x(x, y)\bar{k} \quad (37)$$

$$\bar{R}_{1y} = \frac{\partial \bar{R}_1}{\partial y} = \bar{\jmath} + z_y(x, y)\bar{k} \quad (38)$$

where z_x and z_y are partial derivatives of $z(x, y)$ with respect to x and y, respectively. Therefore,

$$\bar{R}_{1x} \times \bar{R}_{1y} = -z_x(x, y)\bar{\imath} - z_y(x, y)\bar{\jmath} + \bar{k} \quad (39)$$

and

$$\hat{n}_1 = \frac{-z_x\bar{\imath} - z_y\bar{\jmath} + \bar{k}}{\sqrt{z_x^2 + z_y^2 + 1}}. \quad (40)$$

Now

$$z_x = \left(\frac{r_1^2}{f^2 - r_1^2}\right)\frac{x}{z} = \frac{x}{(\eta^2 - 1)z} \quad (41)$$

$$z_y = \left(\frac{r_1^2}{f^2 - r_1^2}\right)\frac{y}{z} = \frac{y}{(\eta^2 - 1)z}. \quad (42)$$

Substitution of Eqs. 41 and 42 in Eq. 40 yields

$$\hat{n}_1 = \frac{-x\bar{\imath} - y\bar{\jmath} - (\eta^2 - 1)z\bar{k}}{\sqrt{x^2 + y^2 + (\eta^2 - 1)^2 z^2}}. \quad (43)$$

With Eqs. 35 and 43, $m_1 \cdot k$ can be found from Eq. 34. In order to determine the quantity $|\bar{R}_2 - \bar{R}_1|$ in Eq. 31, one notes that

$$\hat{m}_1 \cdot \hat{k} = \frac{(\bar{R}_2 - \bar{R}_1) \cdot \hat{k}}{|\bar{R}_2 - \bar{R}_1|} = \frac{r_2 - z}{|\bar{R}_2 - \bar{R}_1|} \tag{44}$$

which, in conjunction with Eq. 34, gives

$$|\bar{R}_2 - \bar{R}_1| = \frac{\eta}{M}(r_2 - z)|\bar{R}_1 - \bar{A}| \tag{45}$$

where

$$M = (\bar{R}_1 - \bar{A}) \cdot \hat{k} + (\hat{n}_1 \cdot \hat{k}) \times \{(\bar{R}_1 - \bar{A}) \cdot \hat{n}_1 - \sqrt{(\eta^2 - 1)|\bar{R}_1 - \bar{A}|^2 + [(\bar{R}_1 - \bar{A}) \cdot \hat{n}_1]^2}\}. \tag{46}$$

With this notation, one finds

$$\hat{m}_2 \cdot \hat{k} = \frac{1}{|\bar{R}_1 - \bar{A}|}\sqrt{M^2 - (\eta^2 - 1)|\bar{R}_1 - \bar{A}|^2} \tag{47}$$

and

$$\delta = d_o - |\bar{R}_1 - \bar{A}|\left\{\frac{b - r_2}{\sqrt{M^2 - (\eta^2 - 1)|\bar{R}_1 - \bar{A}|^2}} + \frac{\eta^2}{M}(r_2 - z) + 1\right\}. \tag{48}$$

When the aperture plane is conveniently chosen to coincide with $\bar{R}_2$, δ in Eq. 48 is much simplified because $b = r_2$. Thus,

$$\delta = d_o - |\bar{R}_1 - \bar{A}|\left\{\frac{\eta^2}{M}(r_2 - z) + 1\right\}. \tag{49}$$

For the present problem,

$$z = r_1\sqrt{1 + \frac{x^2 + y^2}{f^2 - r_1^2}}$$

$$M = z + (f - \epsilon) + \frac{(\eta^2 - 1)z}{\sqrt{\eta^2 z^2 - r_1^2}} \times \left\{\sqrt{\frac{[r_1^2 + z(f - \epsilon)]^2}{\eta^2 z^2 - r_1^2} + (\eta^2 - 1)(z^2 - r_1^2) + (z + f - \epsilon)^2} - \frac{r_1^2 + z(f - \epsilon)}{\sqrt{\eta^2 z^2 - r_1^2}}\right\}. \tag{50}$$

The aperture phase distribution has been computed for the following case:

$$\eta = 1.5 \quad r_1 = 2.8\lambda \quad d \text{ (aperture diameter)} = 20\lambda$$

From the above, one finds:

$$f = \eta r_1 = 4.2\lambda$$

$$\frac{f + r_1}{d} = (\eta + 1)\frac{r_1}{d} = 0.35$$

$$r_2 = r_1\sqrt{1 + \frac{d^2}{4(\eta^2 - 1)r_1^2}} = 9.37230\lambda$$

$$r_2 - r_1 = 6.57230\lambda.$$

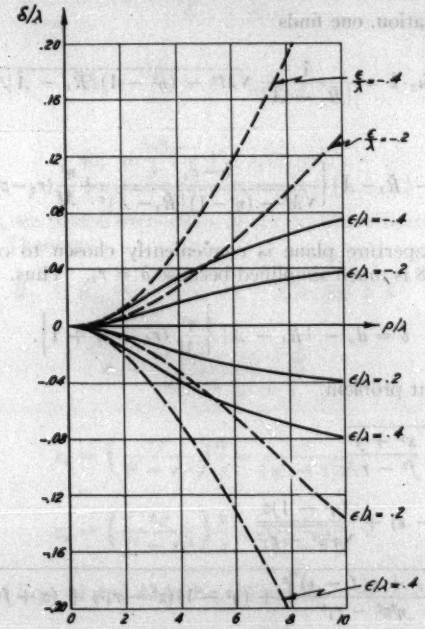

FIG. 3. Aperture phase curves.

Hyperboloidal lens ——
Paraboloidal reflector - - - -

The phase-error curves for $\epsilon = \pm 0.2\lambda$ and $\pm 0.4\lambda$ have been plotted in Fig. 3. The point source is moved from the focal position toward the lens when ϵ is positive and away from the lens when ϵ is negative.

For comparison purposes, the phase-error curves for the same ϵ values are also computed for a paraboloidal reflector with diameter $d = 20\lambda$ and focal length $f_p = 7\lambda$, or $f_p/d = 0.35$. The formula for the phase error in this case has been derived previously (1). An examination of Fig. 3 reveals two points of practical interest: First, the phase-error curves for a defocused paraboloidal reflector are nearly parabolic (quadratic) in nature. This property forms the basis for the feasibility of simulating Fraunhofer radiation patterns in the Fresnel region by suitably defocusing the source (3). Proper defocusing in the reflector case could then give rise to a quadratic phase variation which would cancel the quadratic phase term that exists in the Fresnel region. Second, the phase-error curves for a defocused hyperboloidal lens all show the existence of a point of inflection; they are definitely not parabolic. An immediate and important consequence of this is that simulation of Fraunhofer radiation patterns in the Fresnel region of a lens would not be possible by simply defocusing the source.

Further calculations indicate that, for hyperboloidal lenses of the same diameter and the same focal length, those with a higher index of refraction, η, will have larger phase errors for the same amount of defocus, ϵ.

6. CONCLUSION

General relationships connecting the incident wavefront, two refracting surfaces, and the refracted wavefront have been analyzed. Formulas have been derived for determining the equation of the fourth when the expressions of the other three surfaces are specified. A practical example which determines the aperture phase characteristics of a defocused, hyperboloidal, dielectric lens is given. The results are compared with the phase characteristics of a paraboloidal reflector with on-axis defocusing. It becomes clear that simulation of Fraunhofer radiation patterns for a lens cannot be obtained in the Fresnel region by the defocusing technique. The general formulas are useful not only for determining the aperture phase characteristics but also for designing the lens surfaces for given incident and refracted wavefronts.

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ON GUIDANCE AND CONTROL REQUIREMENTS IN ASTRONAUTICS*

BY

ROBERT E. ROBERSON[1]

ABSTRACT

The question of what can be said about astronautical guidance and control requirements in general, without reference to specific missions, is explored. Mission characterizations are developed which give some broad indications of performance requirements trends. A "network" point of view toward trajectories is suggested which may have important implications to the establishment of classes of functional requirements. Some general conclusions are reached about the qualitative degree to which requirement studies must be expected to be mission-oriented.

INTRODUCTION

The requirements for the performance of guidance and control systems for astronautical applications are not always easy to determine. However, if enough details about the mission are assumed, it is possible to decide on more or less rational grounds what these requirements should be. This would be called a mission-oriented study of requirements.

For example, the operation might concern a passive, plate-type satellite for reflecting communication signals from point to point on the Earth. Here it is necessary to control the plate's orientation so that its normal always bisects the angle between the rays to the ground stations. Precise requirements can be very sensitive to details of the operational concept. In this case, one might keep orientation fixed relative to the Earth if no drift of satellite position were permitted, the latter effectively being a requirement on path control and precise placement in orbit, or one might equally well relax the placement requirements and allow the satellite to drift, the required orientation reference direction gradually changing. The results are two quite different sets of requirements.

Mission-oriented requirement studies are not always feasible. In early considerations of possible methods for guidance and control, it is important to get some idea of performance needs before operational details are crystallized well enough to permit a detailed investigation. Moreover, if detailed assumptions are made, there may be limited carryover to the new problem if even one small change occurs in the actual

* The work on which this paper is based was sponsored by the Flight Control Laboratory, WADC, under Contract AF 33(616)-5975 with the Autonetics Division of North American Aviation, Inc.

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operation. A far preferable approach would be to characterize the mission so that requirements can be generated at least to some degree for a whole class of possibilities at the same time, and so that the results are not too sensitive to all of the assumed details. Such requirements should suffice for preliminary studies and may be supplemented by more specific analyses as the operational concept becomes firmer.

So a basic and very important question in this area is: What can be said about astronautical guidance and control requirements *in general*, without reference to specific missions? More particularly, can we find classes of missions or operations which tend to lead to roughly the same kind of requirements?

The present paper is a partial report on a study addressed to this question. The answer, as might be expected, is not definitive. Still, it turns out that some broad conclusions can be stated which may lead the way to general performance requirements studies for certain mission classes. In addition, a provocative point of view toward the characterization of missions by their trajectories is developed which may have important consequences in functional requirements studies. In effect, the paper suggests some "Handles" with which one can hope to grasp the thorny problem of general guidance and control requirements.

I have discussed the inherent nature of the requirements and have touched upon the qualitative factors which influence their establishment in two previous papers (**1**, **2**).[2] It is made clear there that requirements begin with the mission, the "thing" or entity for which they are to be formulated. So the first step of the present paper is to consider missions within the framework of a rational process of system design. The system design philosophy, often implicit, is reviewed and it is shown how the process leads conceptually to the establishment of guidance and control requirements. The second major topic is how missions might be characterized conveniently from the points of view of both performance and functional requirements. The final major section summarizes the conclusions as to what it is reasonable to expect "in general" and what probably cannot be done outside the framework of mission, operational concept, and system details.

SYSTEM DESIGN PHILOSOPHY

Astronautical Missions

To describe a mission completely one must answer five elementary questions about it: What, why, how, where and when is it to be done, each at the highest possible level of generality. Examples of what a mission is to accomplish, stated at this level of generality, might be "to use a space vehicle to gather scientific information" or "to establish a lunar base." But these by themselves are incomplete characterizations; certainly if requirements are to be developed, at least the reasons for the

[2] The boldface numbers in parentheses refer to the references appended to this paper.

mission must be included. And these should be *true* purposes, not ostensible purposes. For the first example above, the inherent purpose might be included by saying "to gather scientific information of a specific type without reference to its ultimate possible use just from scientific curosity." Frequently, the inherent purposes will be unknown or subject to contention, so the thing I wish to stress here is the danger of losing sight of *any* of the logically possible "whys." One simply must cope with the proliferation of cases that arise, implicitly or explicitly. This is perhaps the first major difficulty in establishing rational requirements, for which a fragmentary statement of purpose will not do.

The "how," or general plan of a mission, is difficult to abstract from the details of the operation used to carry it out. Nevertheless, it is important to recognize that different levels of generality are involved. An example here might be a mission described as follows. One or more Earth satellites ("what") are to be established as space vehicle navigation aids ("why") by lengthening the baseline available for radio guidance ("how"). The kind of "how" details that would be relegated to the operational concept (discussed later) would be such questions as total number of satellites used and their orbit elements.

The only difficulty with the "where" question is the common tendency to be too specific about the entire space trajectory to be followed, forgetting that the only truly relevant constraint may exist only within a localized region—in short, to characterize the whole mission by "where." For example, one hears about the "mission" of "circumnavigating the moon and returning to a braking ellipse around the Earth." This tells the region of space to be traversed, to be sure, but for developing detailed performance requirements, it is no mission at all, only a fragment of one. Another hazard of this point of view is the undue emphasis it sometimes gives trajectory studies. On the other hand, there is some justification for such mission description. We shall see later that the "where" question is indeed the proper characterization for establishing functional requirements on the guidance and control system. But it should be emphasized that such a characterization has limited applicability.

These questions of "what," "why" and "where" imply minimum instrumentation weight, power, communication needs, en route propulsion and the like. The question of "when" is important because it reflects the state-of-the-art at the time a system must be constructed to do the job, thereby providing a population of potentially available subsystems to combine to accomplish the mission. If this ingredient "when" is absent from a mission characterization, one again must recognize the multiplicity of possibilities that are implied.

Once a mission is defined this way, the next logical question is how to design a system to carry it out. Various descriptions of system design philosophy exist, such as (3), but for present purposes it is in-

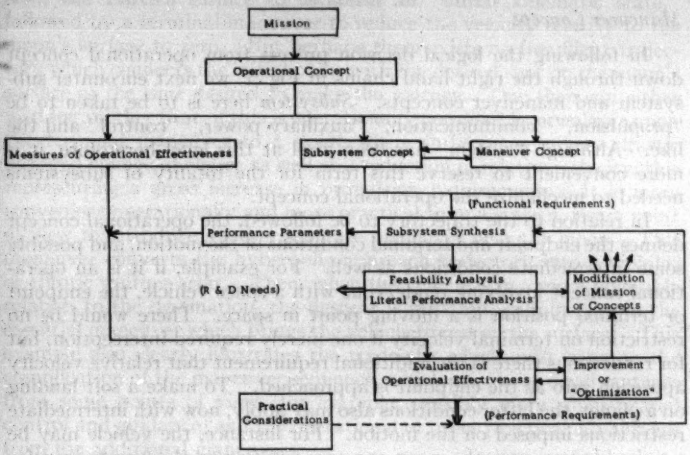

FIG. 1. Logical relationship between steps of astronautical system design.

structive simply to regard the steps of the procedure as steps down from the highest level of generality to a low enough level to permit a physical design. The sequence is illustrated in Fig. 1, and the remaining discussion in this section will refer to the steps shown there.

Operational Concept

The operational concept, often called the "over-all system concept," is the detailed plan of actions to be performed in accomplishing the mission. Although it is developed specifically in view of the mission characteristics, a multitude of operational concepts may apply to the same general mission. The operational concept may encompass such decisions as how many vehicles are to be used, what nominal geometric relationship they bear to one another, and what kind of primary measurement or sensing is to be done from a vehicle. In the use of a set of specially spaced equatorial satellites for communication, for example, the operational concept would dictate whether they are to be individually maneuvered to keep their nominal relationship or whether gaps that open in the nominal pattern are to be plugged with additional satellites. In the case of rendezvous for inspection of a derelict satellite, the concept would dictate whether the inspecting vehicle "pulls alongside" or whether it keeps its distance and sends over a small instrumented probe to do the inspecting. This kind of question, then, forms the area of operational concepts.

Maneuver Concept

In following the logical decision process from operational concept down through the right hand chains in Fig. 1, we next encounter subsystem and maneuver concepts. *Subsystem* here is to be taken to be "propulsion," "communication," "auxiliary power," "control" and the like. Although "system" has been used at this level heretofore, it is more convenient to reserve this term for the totality of subsystems needed to mechanize the operational concept.

In relation to the trajectory to be followed, the operational concept defines the endpoint and terminal conditions of the motion, and possibly some intermediate conditions as well. For example, if it is an operational concept involving rendezvous with a space vehicle, the endpoint or terminal position is a moving point in space. There would be no restriction on terminal velocity if one merely required interception, but for rendezvous there is the additional requirement that relative velocity approach zero as the endpoint is approached. To make a soft landing on a planet, the latter conditions also may apply, now with intermediate restrictions imposed on the motion. For instance, the vehicle may be required to pass near the moon or a space station on the way to transfer persons or information, or even energy by methods of power beaming. If the interplanetary vehicle were an ion-propelled system, it would have to establish some kind of initial Earth orbit and an orbit about the planet in addition to the ultimate terminal conditions on landing.

All of these operational considerations imply conditions on the trajectory to be followed, hence, some powered maneuvers. For the low thrust vehicle the entire trajectory may be one of powered maneuver. In the other ballistic cases, the maneuvers may be by means of intermittent thrust. Effectively, the operational concept defines a certain minimal sequence of free-flight trajectory segments separated by periods of powered flight, which may be called a *maneuver sequence*, understanding that the vehicle is in a ballistic state between maneuvers. Or we can take the opposite point of view and speak of a *sequence of kinematic states*, understanding that maneuvers separate the states. Temporarily we will define a *kinematic state* as travel along a free (unpowered) trajectory in a prescribed force field, starting from a given position at a given time with a given initial velocity. However, the operational concept itself does not completely define the maneuver sequence or kinematic states. In one example above, some requirements for maneuvers are introduced by the nature of the subsystems used as well. *Maneuver concept* means the choice of a complete maneuver sequence on the basis of *all* considerations.

One factor in its formulation is operational practicality or convenience as opposed to the intrinsic requirements of the operational concept. For example, consider an operation involving a soft landing on a prescribed spot on the moon. The minimal set of maneuvers is an ascent

from the Earth's surface to establish an "initial kinematic state," followed by a terminal maneuver to reduce the velocity relative to the moon's surface to zero. But perhaps there exists no free flight trajectory which connects the initial point with the desired spot on the moon's surface at the time desired to make the journey. One then can either wait for another time, if this is operationally permissible, or enlarge upon the terminal maneuver to a point where it becomes a very drastic powered change of course as well as a reduction of relative velocity, this representing a gross increase in propulsion requirements. It is more convenient operationally and more practical from a system design point of view to introduce another separate maneuver near the moon. This maneuver converts the hyberpolic approach trajectory into a circular or elliptic waiting orbit. Then, when the vehicle is in a more favorable relation to the destination, the descent is initiated, culminating in the essential maneuver which brings the vehicle to rest on the surface. This example also clearly illustrates the trade-offs which exist between the complication of the maneuver sequence, which may have disadvantages from some points of view (for example, possibly more sophisticated control and guidance) and the operational or other practical advantages from the additional maneuvers.

In general, maneuver concepts are affected more by considerations of propulsion practicality than operational practicality. Transition orbits, waiting orbits, and the like are all used to "optimize" the trajectory from the propulsion point of view. Indeed, this problem has been the very core of most past astronautical investigations involving trajectories and maneuvers. Examples given above suffice to illustrate the point, although many others could be given. Considerations of control and guidance practicality in relation to the maneuver sequence have been recognized much more recently and have received correspondingly less attention. However, they may be extremely important in practice. A case in point is whether midcourse maneuvers are needed. This depends on the guidance and control accuracy available during ascent from the Earth and on the difficulty of obtaining the necessary information on the basis of which such maneuvers can be made. To be sure, it also depends on propulsion available for later maneuvering near the destination and the operational importance attached to a precise realization of the nominal terminal conditions. Nevertheless, guidance and control considerations play a central role. Other examples might be cited in connection with the use of waiting orbits in order to improve guidance accuracy by acquiring additional information, or maneuvering into a specified plane (for example, the plane of the ecliptic) in order to simplify the acquisition of interplanetary guidance information.

The choice of a maneuver concept is more than merely specifying the qualitative nature of a set of maneuvers or even the times at which they should be performed and the changes in kinematic state that they should

result in. It also is the generation of a nominal steering program to be followed while the powered maneuver is taking place. Except for the ascent phase, this question has received relatively little attention, probably because it is an easy one to answer superficially for ballistic flight outside the atmosphere. Having determined the change of the kinematic state that is desired (for example, the transition from a certain elliptical orbit to a circular orbit), one knows the direction and magnitude of the impulsive vector velocity change that would be required to effect the change at any instant. Since high thrust rockets give a quasi-impulsive velocity change, the steering program consists merely of pointing the rocket (in practice, often pointing the vehicle) in the direction of the desired velocity change and keeping it in that direction during the relatively short period of thrust.

The question gets much more difficult for cases where the thrust is of long duration. The low-thrust interplanetary vehicle is an extreme case in which the whole path of the vehicle, at least between waiting orbits, is determined by the steering philosophy that is used. *The central problem of low thrust trajectories is the choice of a steering program.* Past analyses of such trajectories often have not given enough thought to this matter. Fortunately, though, it has become more widely recognized in the recent work of Rodriguez (4), Levin (5) and others that control and guidance considerations may be the most important factor in the choice of a steering program for the low thrust case.

Subsystem Concepts

The choice of a *nominal* steering program for each powered portion of flight is a part of the maneuver concept area, although it is a choice made in view of control, propulsion and other practical considerations. The choice of a steering philosophy for use when *deviations* have occurred from the nominal trajectory is an entirely separate matter. This is variously called a *guidance concept* or *control concept.*

Subsystem concepts in general concern the selection of subsystem types and modes of operation. The nature of the booster used, the propulsion type and its capacity, communication and information storage subsystems, the source and amount of auxiliary power, the control and guidance type and philosophy of operation would be typical questions. There is a tremendous interplay among these factors, and no single choice can be made except in view of all the others, so that complete decisions are reached by the interactive process which is usual in the design of complex systems. Here, we focus on the control and guidance area, recognizing its sometimes sensitive dependence on all of the other subsystems.

The word "concept" as used here refers to a philosophy or mode of operation. Control and guidance concepts concern two things: first, how in principle a vehicle is made to follow a desired or nominal path;

second, how, again in principle, the effects of deviations from this path are corrected or compensated. These concepts do not concern such questions as whether position is to be determined by optical triangulation or by radio phase difference measurements; whether velocity is to be determined by differencing position measurements or by doppler radar; whether orientation is to be sensed by an optical horizon scanner or with reference to a stellar-monitored inertial platform; whether orientation is to be controlled by spin stabilization, jet thrust, or accelerating internal wheels.

They concern such questions as whether position or velocity measurements are needed or whether some other function is better sensed (as in the simple lunar trajectory correction scheme of (6)); whether one uses the measurements to make an immediate course correction or merely predicts new terminal conditions and makes any necessary corrections at the end; whether, once deviations from a nominal path are detected, to make efforts to return to that path or merely to arrange things so the deviations have returned to zero by the time the terminal conditions are reached; and, in the latter case, just how one "arranges things." Most of these questions are closely related to the steering program, but special emphasis is on the behavior of an actual system which, because of various imperfections and disturbances, does not follow the nominal path that had been selected.

Functional requirements are already established when the concepts discussed above are crystallized, so one knows what each subsystem is to accomplish qualitatively. This makes it possible to draw a functional block diagram of its operation. In the case of the control subsystem, this shows the nature of the sensing inputs needed, processing operations on the sensed equations, signals to the control actuators, and the output of the control subsystem—vehicle combination in terms of path and attitude variables. There is no latitude in this functional connection diagram at this point, possible variations having been removed in crystallizing the subsystem concept. However, there still is the problem of selecting the types of physical elements to be visualized as occupying the various blocks of the diagram.

This is part of the problem of synthesis. If a sensing input is needed which provides absolute attitude of a vehicle relative to some specified reference, the synthesis process decides whether the information is to be obtained from an optical horizon scanner, a radio method, or in some other way. In short, synthesis chooses from among the many logically possible methods which can be used to mechanize the block diagram, one which is "best" from practical and performance considerations. But in addition, synthesis means choosing the detailed parameters for each element. Examples might be the damping and spring constants for gyroscopic instruments, servo loop gains, focal lengths and apertures of optical systems, operating frequencies for radio methods, and the like.

Synthesis is again a step-wise process. After a preliminary choice is made, the performance of the subsystem is evaluated, first on the grounds of general feasibility and then on the basis of quantitative behavior as measured by a number of *performance parameters*. This analysis process may be expected to result in any of three kinds of action. First, if the synthesized subsystem and all obvious variations of it are not feasible, new research and development needs are indicated. Second, performance inadequacies may suggest modifications of these goals or of the operational concept on which the astronautical system is based. This often is an important possibility, for many aspects of the operational concept might have been quite arbitrary, chosen merely for definiteness rather than because of any commanding inherent desirability. In such cases, it does no harm to modify the operational concept in some respects. Third, the subsystem performance in terms of its own natural performance parameters can be used to evaluate the system effectiveness in terms of performance parameters, which more naturally express the operational goals.

This brings in the other chain descending on the left hand side of Fig. 1. The definition of the mission carries with it explicit intrinsic mission goals. The operational concept further translates these goals into more specific and quantitative terms. Certain variables called *measures of operational effectiveness* or *operational performance parameters* will exist, whose values determine the degree to which the operational goals are achieved. The first step in a rational evaluation process is one or more of these measures of effectiveness. A single variable is preferable, for if there is more than one it is often found that the several measures respond differently to variations in operational details. But often there is no measure of effectiveness, so there is no way of objectively balancing conflicting factors in practice except by subjective judgment as to what is "best."

The important thing to remember is not to focus on any one measure simply because it is simple and "optimize" performance relative to it, ignoring other important measures of effectiveness. The dangers of suboptimization of this kind are nicely illustrated by Hitch (7).

Each subsystem will also be characterized by a certain set of parameters or variables which measure its performance. Examples are "accuracy" and "reliability." If the measures of operational effectiveness are related to the subsystem performance parameters, and if the proposed subsystem is analyzed in terms of these parameters, one can actually evaluate the operational effectiveness of the system. Improvement efforts and "optimization" (actually suboptimization) may result from this, as may some modification of the mission or the various concepts—operational, subsystem and maneuver.

In this whole process one need not use numerical values for the subsystem parameters—effectively, need not have done the part of the

synthesis that results in definite parameter value choices, but merely the part that selects element type. Subsystem structure is essential, of course, in order to do a performance analysis at all, even in literal terms. Thus, one ideally has in the evaluation stage a literal relationship between operational effectiveness and the subsystem performance parameters. If one assigns (probably subjectively) a level of satisfactory operational performance, the result is one or more constraints on the subsystem performance, from which can be derived a set of subsystem performance requirements. These, then, feed back into the portion of the system synthesis which involves the choice of detailed element characteristics.

MISSION CHARACTERISTICS

Mission Classes

Previous remarks have emphasized the importance and difficulty of properly defining mission and operational concept, and the fact that this is an essential preliminary to the formulation of maneuver and subsystem concepts; hence, to the development of guidance and control requirements. Since a very large number of specific mission-concept possibilities exist, it is essential to narrow them down if any general conclusions about such requirements are to be derived. This suggests that an attempt should be made to construct a relatively small number of mission classes in order to come to grips with requirement generalities.

There is no unique or "best" way to develop such classes, but the approach followed in the present study has proved to be a useful one.

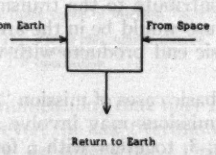

FIG. 2. Structure of astronautical mission "activity types."

The point of view taken in it is in terms of "activity types," loosely related to the "what" and "how" factors discussed previously. Any mission involves an astronautical system which "does something," and there are only a few things that can be done, speaking at a high level of generality. Basically these are the transmission of something and the processing of something, where the "something" must consist of persons, other material things, and non-material things embracing ideas, messages, etc. (all of which may be lumped together as "information").

Thus, any astronautical mission involves as possibilities the transmission of persons, material or information into space (not necessarily

simultaneously); the acquisition of new material or information from space; the processing on any or all of these things; and finally, the redirection of something to Earth. Schematically, activity types can be represented as shown in Fig. 2. The transmission is shown as arrows, the processing as a box. Each arrow is to be labeled with H (human), M (material), I (information) or a combination as appropriate. We rule out the case where *nothing* is sent back as inherently improbable. Even for a manned deep-space probe, for example, in which the humans are not expected to return within their lifetimes, the mission is not likely to be undertaken unless some information is forthcoming. Another logical possibility of this kind is to send out material with no expectation of any kind of return, such as in an operation which might be described as "making space a garbage dump" (for example, for radioactive material disposal). Again, though, this case is explicitly omitted from the categories.

All permissible remaining cases can then be developed using a few basic rules. Humans can neither be sent out nor returned without material in the form of a space vehicle, but this point will be understood and all combinations of man and vehicle will be denoted simply by H. We assume no H can be sent out unless it also returns. We assume no I can be sent out unless accompanied or preceded by men or material to do something with it. The use of the symbol M denotes that the basic payload of the space vehicle is instrumentation or other non-human substance (including, however, other life forms). Finally, it is convenient to assume that no M or I is accepted from space unless it is returned to Earth. Strictly, of course, it might be taken in by the space system and made to contribute to the transmission or processing processes. However, that use would be in the area of operational concepts rather than that of basic end products with which the mission is concerned.

There are just nine basic cases of mission "activity types," although complex multipurpose missions may involve a superposition of types. They are shown in Fig. 3, together with a few of the activities which each might include. Some of these cases form "natural" groupings (1 and 4, 2 and 5, 3 and 6, 7 and 8 and 9). Moreover, in seeking to illustrate each case, one is led repeatedly to the same descriptive words to characterize the activity type. This motivates the composition of basic mission classes from these descriptive terms.

Thus, one is led to distinguish three classes:

I. Exploration and Scientific Data Gathering (3, 6)
II. Observation and Communication (7, 8, 9)
III. Transport, Weapon Delivery and Supply (1, 2, 4, 5).

Figure 3 shows the close relationship between some of these words (that

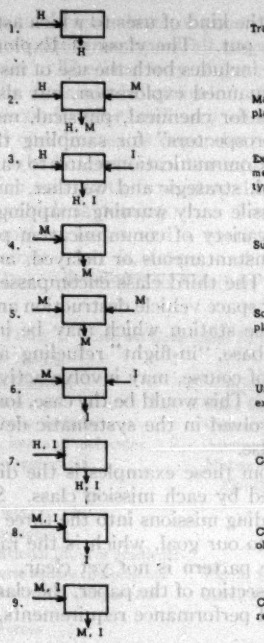

FIG. 3. The basic cases of mission "activity types."

H = human plus support systems
M = material
I = information

is, observation and communication) that may not be evident from the words themselves. However, the exact terminology used for the classes is to be considered only as suggestive. Utilization of resources is lumped with Class III, anticipating its close relationship with transport problems when viewed in the light of operations and trajectories required. It will be important to avoid semantic difficulties by the use of such phraseology as "Class II missions are typified by observation and communication activities" instead of the dangerous "Class II is observation and communication missions." It is understood that if another activity type not related to observation and communication still resembles cases 7, 8 or 9 of Fig. 3, it should be lumped with Class II. In using these classes, it is important to remember two other things as well. First, each class represents a variety of missions. Second, each mission gives rise to a variety of operational concepts.

It is easy to give examples of the kind of uses to which astronautical systems of each class might be put. The class of Exploration and Scientific Data Gathering clearly includes both the use of instrumented probes to various locations and manned exploration, but also includes such things as space laboratories for chemical, physical, medical and biological studies, and "lunar prospectors" for sampling the moon's crust. The class Observation and Communication relates to various kinds of reconnaissance, such as tactical, strategic and weather, inspection of enemy space vehicles, ballistic missile early warning, mapping of Earth, moon and planets, and a large variety of communication possibilities (point to point, point to area, instantaneous or delayed, and various combinations of these factors). The third class encompasses strategic bombardment, missile, satellite or space vehicle destruction and neutralization, ferry to and from a space station which may be in use as a way-station, a storage depot or a base, "in-flight" refueling, and similar activities. Complex operations, of course, may involve activities from all three of the categories at once. This would be the case, for example, for the complex of operations involved in the systematic development of a manned lunar observation base.

One thing that stands out from these examples is the diversity of applications which are represented by each mission class. So, in one respect, it would appear that dividing missions into the three suggested classes does not bring us nearer to our goal, which is the rational development of requirements. The pattern is not yet clear. However, as will be brought out in the final section of the paper, the classes above do have significance in the area of performance requirements.

Trajectory Classes

Although the above mission classes provide some general guides to performance requirements, the diversity of activities encompassed by each class makes it difficult to generalize in the same way about functional requirements. Therefore, we are led to seek further characterizations which will mirror the function requirement differences which arise from mission to mission.

It is shown in the next section that the only way the mission affects functional requirements on control is through the sequence of kinematic states and their separating maneuvers through which the vehicle must pass in order to accomplish the mission. Anticipating this result, we conclude that the sequence of kinematic states and maneuvers is precisely the characterization sought. It will be developed here primarily from the point of view of vehicle path, although many of the considerations also apply to attitude.

Kinematic state and maneuver had been defined, but we have not yet considered how one state can be distinguished from another, other than by a whole set of orbit elements. For present purposes, we need

to advance to a higher level of generality and seek classes of trajectory phases which will encompass all cases but keep the development to a tractable size. The best way of generating such classes seems to be in terms of the dynamical description of the system, which consists of six or more second order ordinary differential equations with suitable generalized coordinates as dependent variables. In their conventional form, the right hand sides of the equations consist of generalized force terms, which arise both from propulsion and the ambient field through which the vehicle is passing.

Three major classes and a total of eight subclasses of generalized external forces (that is, forces and torques for the usual choices of generalized coordinates) are listed in Table I, together with examples.

TABLE I. *External Generalized Forces on an Astronautical System.*

SOURCE	EXAMPLES	
1. Momentum Transfer	Incident Momentum	Expelled Momentum
1.1 Radiation	Solar Radiation	r.f. and Thermal Radiation
1.2 Sub-atomic particles	Cosmic Rays	
1.3 Atoms, ions and molecules	Atmosphere	Chemical Fuel and Ion Rockets
1.4 Particle aggregates	Meteorites, Collision with Planet	
2. Ambient Fields		
2.1 Electromagnetic	Earth's Field	
2.2 Gravitational (fluid or gradient)		
a. Inverse square components	Earth, Sun, Moon, Other Bodies	
b. Perturbation components	Earth's Oblateness	
3. Apparent Forces		
3.1 Non-inertial reference frame	Coordinates Rotating about Earth with Moon or the Vehicle	
3.2 Vehicle geometry changes	Swiveling Antenna	

Note that exactly the same effects sometimes can give rise to parametric excitation as well as "external" driving terms in the differential equations of motion. If one considers the actual progress of an astronautical vehicle through its dynamical history, it becomes clear that it passes successively through time periods or regions within which there is just one dominant generalized force, perhaps two or three. This suggests identifying a single kinematic state or maneuver with the dominance of a specified set of forces, a procedure which makes somewhat more precise the previous definition of such states as periods within which the equations of motion have a time-invariant structure. For, if all the force laws are inserted at the beginning and parameters are included to describe the times at which some forces are turned on and off (for example, propulsion), the equations necessarily have this invariant structure throughout the history of the system, and the original definition breaks down.

The new characterization is not flawless, for a major gap concerns the question of what constitutes "dominance" of some forces.[3] Pragmatically, it can be settled by deciding which are essential in obtaining a direct solution of the equations and which can be handled by perturbation methods on generating solutions. Obviously there is really a gradual transition between states in many cases. However, as long as it is understood that a test or criterion must be made explicit, the point will not be troublesome in the use we make of these notions below.

So now we have a set of building blocks for the sequence of kinematic states and maneuvers. They are simply sets of dominant generalized forces. How many building blocks are there? Suppose there is a total of F kinds of force terms included in the dynamical equations. The number of ways r forces can dominate is $\binom{F}{r}$ and the total number of cases or building blocks must be $\sum_{r=0}^{F} \binom{F}{r} = 2^F$. Since F must be at least 8 (there are this many subclasses in Table I), we see that there are at least $2^8 = 256$ potential building blocks or elementary states. Actually, if we focus on path alone, most cases of major astronautical importance can be taken care of by the following seven sets of dominant forces:

1. Rest state, no external forces
2. Air drag
3. Single gravitating point mass
4. Two gravitating point masses
5. High thrust in the atmosphere
6. Impulsive or quasi-impulsive high thrust[4]
7. Continuous low thrust.

In all real cases, of course, there will exist perturbations of the derived motion obtained under the assumption that the dominant forces alone are acting, but these are more important to performance than to functional requirements.

Next, consider the combination of elementary states into complete maneuver sequences. The discussion is based on the seven items above, although it is clear that similar considerations hold for more sophisticated cases. The first topic is the transitions that may exist among items. For example, motion in the field of a single gravitating point

[3] Note that we may speak of the dominance of *many* forces, meaning in effect that we cannot really find any forces which dominate in the usual sense of the word.

[4] Includes relatively short continuous thrust periods that might be used for soft landing or rendezvous. The characteristic here is that the duration is short enough that the other forces do not *change* significantly during the time. Thus, we do not have to worry about combination cases of item 6 with item 4, etc.

mass (item 3) may culminate in a rest state (item 1) if the central body has no atmosphere and the vehicle bumps into it, in three-body motion (item 4) if the two-body motion carries the vehicle too near a third body, in motion with air drag (item 2) if the central body has an atmosphere and the vehicle passes through it, or it may be terminated by impulsive or continuous thrust (item 6 or 7). We denote this by the symbolism $3 \rightarrow 1, 2, 4, 6, 7$. Each state can be analyzed in the same way, with the result shown in the first column of Table II. Not all logical possibilities are shown, only those which one might consider most likely. For example, unpowered motion in the atmosphere is shown to culminate only in rest or in free two-body motion, whereas, in fact, one can visualize operations where low or high thrust is applied to carry the vehicle back outside the atmosphere. However, the development is intended to be suggestive rather than definitive, and the other possibilities can be added whenever desired. One further point is that some logical impossible or unreasonable sequences will be generated from these results.

TABLE II. *Elementary State Transitions for Trajectories.*

Consequences	Antecedents	Difference Equations for Numbers of States**
$1 \rightarrow 5$ at first step only	$1, 3, 4, 5 \rightarrow 0$	$0_n = 1_{n-1} + 3_{n-1} + 4_{n-1} + 5_{n-1}$
$\rightarrow 0, 5, 6$ thereafter*	$2, 3, 6 \rightarrow 1$	$1_n = 2_{n-1} + 3_{n-1} + 6_{n-1}$
$2 \rightarrow 1, 3$	$3, 5, 7 \rightarrow 2$	$2_n = 3_{n-1} + 5_{n-1} + 7_{n-1}$
$3 \rightarrow 0, 1, 2, 4, 6, 7$	$2, 4, 5, 6, 7 \rightarrow 3$	$3_n = 2_{n-1} + 4_{n-1} + 5_{n-1} + 6_{n-1} + 7_{n-1}$
$4 \rightarrow 0, 3, 6, 7$	$3, 6, 7 \rightarrow 4$	$4_n = 3_{n-1} + 6_{n-1} + 7_{n-1}$
$5 \rightarrow 0, 2, 3$	$1 \rightarrow 5$	$5_n = 1_{n-1}$
$6 \rightarrow 1, 3, 4, 7$	$1, 3, 4, 7 \rightarrow 6$	$6_n = 1_{n-1} + 3_{n-1} + 4_{n-1} + 7_{n-1}$
$7 \rightarrow 2, 3, 4, 6$	$3, 4, 6 \rightarrow 7$	$7_n = 3_{n-1} + 4_{n-1} + 6_{n-1}$

* 0 Signifies termination of sequence.
** Initial conditions $1_0 = 1, 2_0 = 3_0 = \cdots = 7_0 = 0$.

For example, if $2 \rightarrow 1, 3$ then the 3 resulting from 2 can go only back into 2 or into one of the items representing thrust application, not into all the items that a 3 can logically go into if its origin is not examined. However, it is easier to accept such cases and weed them out at the end than to complicate matters at this point by setting up iron-clad transition rules which take account of all previous history.

Sequences of states now can be built up as shown in Fig. 4. Beginning with state 1 (rest) at the zeroth step, which is necessarily followed (if at all) by state 5 at the first step, we can diagram all of the states which are consequences of 5 at the second step, and so on. The result is a tree, each branch of which represents a complete trajectory of some kind, and therefore a mission and operational concept as seen from the viewpoint of functional control requirements. The analogy with network trees with the elementary states as network elements is too striking to ignore, but an exploration of this point is just beginning.

How many trees are there after n steps? It is obvious from Fig. 4 that there are a lot. It is convenient to rewrite the first column of

Table II as the second column in which the results are arranged to show what states can go into a given state. Now representing the *number* of 3-states at the end of n steps by 3_n, and similarly for the other items, one can construct difference equations for the number of each kind of state present at the end of n steps. These are shown in the third column of the table. The total number of branches in the tree is $b_n = 1_n + 2_n + 3_n + 4_n + 5_n + 6_n + 7_n + \sum_{k=1}^{n} 0_n$, the latter summa-

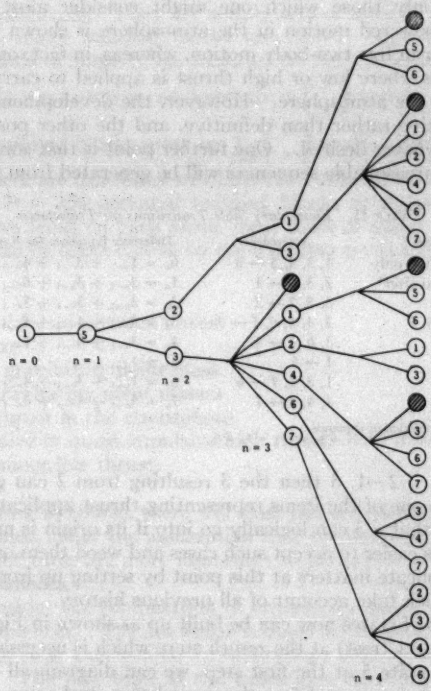

FIG. 4. Four-step trees of trajectories.

tion to pick up any branches terminating before the n^{th} step which would not otherwise be counted if we focused on the n^{th} step alone.

The set of difference equations can be reduced to a single equation in any one of the types of states and is easily solved in principle. This can be done for all states and b_n found. Unfortunately the process requires the solution of a sixth order algebraic equation, but this is no

bar to obtaining a numerical solution if it is desired. (This is one case where a solution of the sixth order case in terms of sixth roots might be expected, since all solutions must have integral values.)

The process can be illustrated by a simple example. Consider the special set of trajectories described by the following process (analogous to Table II): $1 \rightarrow 5$ ($n = 0$), $\rightarrow 0$ thereafter; $2 \rightarrow 1$; $3 \rightarrow 2, 4, 6$; $4 \rightarrow 0$ (that is, if vehicle escapes from the Earth the mission is considered terminated); $5 \rightarrow 3$; $6 \rightarrow 3, 4$. The corresponding difference equations are:

$$\left.\begin{array}{ll} 0_n + 1 = 1_n + 4_n & 4_{n+1} = 3_n + 6_n \\ 1_{n+1} = 2_n & 5_{n+1} = 0n \geq 1 \\ 2_{n+1} = 3_n & \quad = 1n = 0 \\ 3_{n+1} = 5_n + 6_n & 6_{n+1} = 3_n \\ (0_1 = 1_1 = 2_1 = 3_1 = 4_1 = 6_1 = 0) & \end{array}\right\} \quad (1)$$

But the solution of these reduces simply to the solution of $3_{n+2} = 3_n + 5_{n+1}$ ($n \geq 0$) or $3_{n+2} = 3_n$ ($n \geq 1$ with $3_1 = 0$ and $3_2 = 1$), namely $3_n = \frac{1}{2}[1 + (-1)^n]$ ($n \geq 1$). In turn, $1_{n+2} = \frac{1}{2}[1 + (-1)^n]$ ($n \geq 1$, $1_1 = 1_2 = 0$); $2_{n+1} = \frac{1}{2}[1 + (-1)^n]$ ($n \geq 1$, $2_1 = 0$); 4_{n+2} 1 ($n \geq 1$, $4_1 = 4_2 = 0$); $6_{n+1} = \frac{1}{2}[1 + (-1)^n]$ ($n \geq 1$, $6_1 = 0$); $0_{n+3} = \frac{1}{2}[3 + (-1)^n]$ ($n \geq 1$, $0_1 = 0_2 = 0_3 = 0$). This implies $b_{n+3} = \frac{1}{2}[3n + 5 + (-1)^n]$ ($n \geq 1$, $b_1 = b_2 = 1$, $b_3 = 3$). Thus, in this simple case the number of branches of the tree grows just proportionally with the number of steps, with a small oscillation. Exactly the same procedure holds in the more general case, but the number of branches grows as higher powers of n. To complete this simple illustration, the first few steps

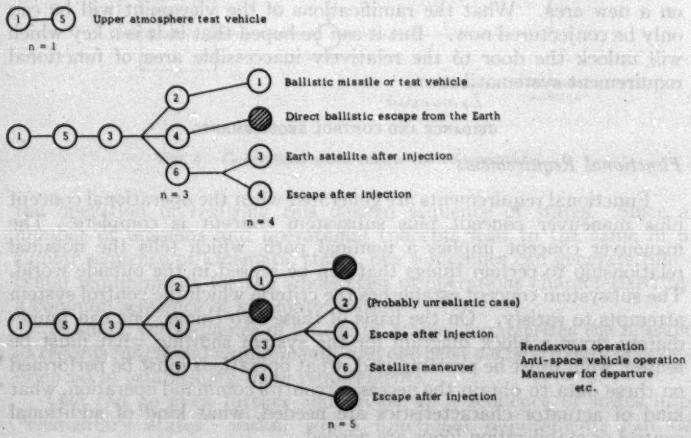

FIG. 5. Trajectory tree derived for a special simple model.

of the trajectory tree are sketched in Fig. 5, with some indication of the type operation to which each might correspond. Some of the behavior mentioned previously is manifested here, such as the quick appearance of a case which probably would not be common in practice. In addition, two of the cases at step five are essentially equivalent. Exploitation of network theory in approaching the general problem by this route probably would help in weeding out equivalences and redundancies.

One more general comment on this path characterization is of practical importance. In considering maneuvers in the field of a single body (where the equations of two-body motion apply), maneuver complexes are built up from segments of elliptic, circular and hyperbolic orbits. Therefore, it may be very useful to break item 3 (in the dominant force list above) into three parts which reflect these possibilities. In effect this generalizes it from an item purely described by force type to one which also makes allowance for three different sets of initial conditions. In fact, it is conjectured that the items should be reconstituted in some way to make a distinction between an impulse which generates a greater eccentricity and one which generates a lesser. Such questions are an important topic for further study.

Having unfolded a new picture of trajectories this far, what do we have? It would appear that in this point of view we have a systematic way of generating complete trajectory descriptions, a way of developing a catalog of logical trajectory classes, a way of interrelating and comparing trajectories to discern common elements, and a potential handle to use in bringing to bear powerful methods of network theory—such as those relating to topological invariance and network equivalence—on a new area. What the ramifications of the viewpoint will be can only be conjectured now. But it can be hoped that in it is a key which will unlock the door to the relatively inaccessible area of functional requirement systematization.

GUIDANCE AND CONTROL REQUIREMENTS

Functional Requirements

Functional requirements are generated when the operational concept plus maneuver concept plus subsystem concept is complete. The maneuver concept implies a nominal path, which tells the nominal relationship to certain things that can be sensed in the outside world. The subsystem concept establishes the criteria which the control system attempts to satisfy. On the basis of these two things, one can immediately draw a block diagram for the system showing what must be sensed, what must be precomputed, what operations must be performed on these data to obtain the necessary control command operation, what kind of actuator characteristics are needed, what kind of additional control or stabilization loops are needed.

A generalized space control and guidance, therefore, takes the form shown in Fig. 6. Note that this is a conventionalized representation of a multi-dimensional situation, and that the single stabilization loop shown actually may be comprised of several loops, as the vehicle may be comprised of several separate parts. When all of these facets, hidden in Fig. 6, are made explicit and specific functional relations are put into the blocks, one has in effect established the functional requirements.

What really determines these requirements? What is the control system *really* trying to do? It is simply trying to establish the path and orientation that is desired for the conduct of the operation—more generally, the sequence of kinematic states through which the vehicle must pass. Because, after all, the "control and guidance" is precisely

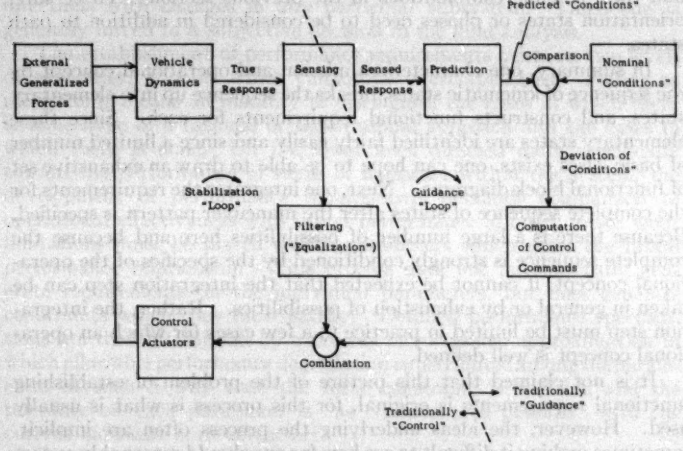

FIG. 6. Generalized space vehicle control operations.

the operation of realizing this desired sequence of states. Since a number of missions and operational concepts lead to the same or a similar sequence of kinematic states, one sometimes can generate functional requirements for the whole class of operations characterized by a common sequence of kinematic states.

In short, the sequence of kinematic states characterizes the mission as regards functional requirements, and one need *not* be strongly mission-oriented in establishing these requirements.

One can go even further and divide up a sequence of states into "elementary states" within which functional requirements can be

established for path control and attitude control. In the case of the path, these elementary states are simply the trajectory phases. The result is a set of functional block diagrams which apply sequentially throughout the operation. By superimposing the diagrams (in practice, one might use transparent sheets), common functions of the elementary states can be discerned and one can construct a composite functional diagram which incorporates all functions needed for the whole sequence of kinematic states. On the hardware level, this commonly is known as "system integration." On the functional requirements level, we may call it "functional integration."

These considerations, which show that one really need focus only on trajectory phases for an attack on the functional requirements problem, were the motivation for the investigation of elementary states and their possible combinations in the previous section. To be sure, orientation states or phases need to be considered in addition to path states.

In summary, one characterizes mission and operational concept by the sequence of kinematic states, breaks the sequence up into elementary states, and constructs functional requirements for each. Since these elementary states are identified fairly easily and since a limited number of basic cases exists, one can hope to be able to draw an exhaustive set of functional block diagrams. Next, one integrates the requirements for the complete sequence of states after the maneuver pattern is specified. Because there is a large number of possibilities here and because the complete sequence is strongly conditioned by the specifics of the operational concept, it cannot be expected that the integration step can be taken in general or by exhaustion of possibilities. Rather, the integration step must be limited in practice to a few cases for which an operational concept is well defined.

It is not claimed that this picture of the problem of establishing functional requirements is original, for this process is what is usually used. However, the ideas underlying the process often are implicit, sometimes making it difficult to see how far one should reasonably expect to carry the requirements formulation for an incompletely crystallized mission. It is felt that making them explicit here is an extremely important preliminary to further investigations of such questions.

Performance Requirements

There are essential differences between functional and performance requirements. The latter are only partly established through the chain (Fig. 1) of subsystem and maneuver concepts. After a functional block diagram is constructed, one proceeds with subsystem synthesis, consisting of filling in functional blocks with specific kinds of physical devices. But simultaneously one develops measures of effectiveness for

the specific operational concept and to establish performance requirements relates these to the results of a literal performance analysis of the synthesized system.

Two points stand out. First, one cannot go a long way in this direction except in view of relatively well defined mission and operational concepts. However, a careful construction of mission classes along the lines of the previous section suggests that there may be broad criteria of effectiveness applicable to certain types of missions, at least for purposes of suboptimization. (These are discussed below.) Second, even when mission and operational concepts *are* well defined, finding a proper measure of effectiveness is an extremely difficult job. It is more usual to find *several* reasonable measures which cannot be combined into one common measure and which change in different directions with changes in any particular subsystem. Thus, one is generally forced to a subjective decision in the final analysis.

The establishment of performance requirements exists on two levels of generality, a point which cannot be overemphasized. Failing to recognize it means an inability to distinguish the cases in which there is some hope of a rational or semi-rational approach and the cases in which there is none. On the lower level directed more specifically toward the performance of items of hardware within the system, the development of performance requirements demands comparison of operational effectiveness with literal performance in order to see what limitations must be placed on the parameters which enter the literal performance relationship. This process does not automatically generate requirements on the individual parameters, but does generate constraints which they must satisfy. Detailed performance specifications are then established by a process of successive approximation in which allowable performance degradation is distributed among the parameters in such a way that the total constraint is satisfied and the tolerance on each parameter is "as consistent as possible" with the current hardware state-of-the-art.

So one sees that even if all of the other steps can be taken, quantitative performance specifications are established by subjective judgment. While a rational approach might exist in concept (by some process of introducing state-of-the-art and development-difficulty considerations in an objective fashion), we are forced to conclude that there is no reasonable hope for a rational approach to performance specifications on this level of generality.

At a higher level of generality the picture is somewhat brighter. For example, using the mission classes developed previously, it is possible to distinguish certain general tendencies about performance requirements. First, for manned missions of all classes, reliability is the performance characteristic that probably would be judged most important and for Class I might be almost a sole determinant of operational

effectiveness. Second, for missions of Class I, path accuracy is probably not of as great importance as we are used to thinking of it for terrestrial guidance systems. Although nominal performance goals would be established, failing to meet them accuracy-wise might be annoying, but probably would degrade the operational effectiveness only a little. For manned missions, path accuracy would be important primarily because of its implications to propellant exhaustion. (Naturally, one can construct isolated examples of operational concept where accuracy or almost any other factor would be all-important, but we are trying to focus on trends here.) On the other hand, because scientific data gathering and exploration goals tend to be early ones, and because early systems will tend to be weight and power starved, both weight and power will be important performance parameters. Finally, we may expect free-flight attitude control accuracy to be relatively unimportant, at least for scientific data gathering (8).

It seems possible to make even stronger statements about Class II, which is characterized by the acquisition, processing and transmission of information. Since the delivery of information to the Earth is the true *raison d'être* of these systems, one is led strongly to conjecture that the best criterion of operational effectiveness is formulated in terms of information loss or degradation. If the control system works perfectly throughout the temporal history of the operation, a certain capability will exist in terms of total expected number of bits of information received on the ground. If the control system behaves imperfectly, the actual number of bits will be less. A measure of control performance in these terms has an extremely important potential, for it can be translated into additional time or additional vehicles required to "do the same job" or into equivalent changes in various aspects of the operational or other subsystem concepts. An approach to rational evaluation of reconnaissance system performance on this basis has been made by the author in unpublished work. However, similar investigations for other operations are strongly indicated.

Now most operational concepts of Class II depend upon the free-flight attitude stabilization of sensors to acquire information and effectors to send it on. It follows that the accuracy of free-flight attitude control probably is a performance factor of major importance together with reliability, both of which affect transmitted information content. Path control also is important for certain operational concepts (for example, those involving precisely placed or spaced satellites), but this seems to be more a characteristic of the particular way chosen to do the job than an inherent one of the job (information handling) itself.

Class III presents a contrast, for here the problem is the transfer of material rather than information and a suggestive measure of operational effectiveness is the probability that the material actually is delivered to within a specified distance of the nominal delivery point

within a specified time interval around the instant delivery is desired. It is evident that this kind of criterion is a measure of effectiveness for bombardment systems, albeit one which neglects some factors such as economics. In the same way, it is at least one measure of what one "really wants to do" in transferring men or supplies.

It also follows that if this is the proper measure of effectiveness, the performance parameters which affect it most significantly are path control accuracy and reliability.

Mixed cases or operations with multi-purpose goals, as always, present a problem. There seem to be only three possible courses of action: hope that the several purposes do not lead to conflict in performance parameters, decide which purpose is "really most important" and design for it, or abandon any attempt toward the development of rational performance requirements and resort to completely subjective judgment.

It is felt that these remarks fairly well establish the limits of what can be said in general about the relation of control performance requirements to astronautical missions without going considerably further into the details of the latter.

However, in addition to further studies based on the general trends, another tool traditional in system design problems can be used. It holds the measures of effectiveness (which perhaps are better called "sub-optimization criteria" to keep their limitations in the fore) constant while varying subsystem performance parameters in various ways. This is the usual *trade-off study* that establishes what a marginal increment in one performance parameter is "worth" by this criterion in terms of another.

One must beware of falsely interpreting the goals of trade-off studies. They are not basically for the rational establishment of "optimum parameter values," although this sometimes is realized as a side benefit, inasmuch as the final selection of desirable operating values for the parameters usually remains a subjective matter even at this level of generality. Rather, the true goal is to suggest ways changes may be made to "improve" the subsystem. When an increase of a certain parameter seems to pay off in terms of a certain criterion, one may consider pushing the subsystem realization in this direction. But before doing it, before actually selecting working values of the parameters, one must see what effect the change has on other criteria and evaluate it in terms of "practical considerations." In short, one does not *really* use trade-offs for "optimization," but as a guide to systematize and partly rationalize the application of intuition and subjective judgment.

One other point must be remembered about trade-offs. Before they can occur, one must have found at least one scheme which will work (that is, satisfy the functional requirements). This is not always easy.

CONCLUSIONS

Although this study has been of a rather general nature, some relatively specific conclusions have been reached. Some of these may be of an obvious nature in retrospect, yet enjoyed only conjectural status before a searching look was taken at the guidance and control requirements problem. It is felt that both the suggested measures of operational effectiveness and the "trajectory tree" point of view which is developed here may be useful in further requirements investigations. Major conclusions are:

1. Functional requirements can be developed under rather general conditions on the basis of elementary kinematic states.
2. Sequences of states can become complicated very rapidly, making the generation of integrated functional requirements quite difficult and much more mission-oriented. However, a network point of view is presented which after further development may expedite the process.
3. Trade-off studies are about the most that can be expected in the way of rational development of quantitative performance requirements without further development of items 4 and 5 below.
4. There may be some measures of effectiveness of rather general applicability in three broad mission classes.
5. Crude trends exist regarding where the burden of performance should be placed in each of these classes.

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THE LIBRARY OF THE FRANKLIN INSTITUTE*

BY

JOSEPH SAMUEL HEPBURN[1]

Previous articles (1)[2] have told of the early history of The Franklin Institute and of the relations its early members had with contemporary organizations and institutions of Philadelphia.

The present article reviews the development and growth of the Library of The Franklin Institute, a story which is recorded chiefly in the manuscript minutes of the Board of Managers and of the Committee on Library. Today the Library is the most complete and largest, in the fields of chemistry, physics, and engineering, between New York and Washington; this is true of books, periodicals, abstract journals, and patents. The size of the collection is shown by statistics of its removal to the present building in 1933 by a professional mover. Forty-one working days were required; and 6329 boxes were used. One library assistant supervised the packing; and three men, supervised by a library assistant, were required to place the books on the shelves. Naturally the collection has increased by additions in the quarter-century which has elapsed since the removal.

The number of books in the Library has grown steadily:

1855	6,022
1874	12,000
1899	50,348
1957	156,413
1959	167,135

On January 1, 1960 the Library also contained 57,536 pamphlets and 18,081 maps.

Currently 1336 periodicals are received. As far back as 1888 the Library had complete sets of over 200 serials. When Frederick Ransome, who became an honorary member of the Institute in 1877 and was the London Agent of the Committee on Library, died in 1893, it was recorded that he had served gratuitously and had aided in completing sets of 31 British journals whose publication began early in that century and still continued.

* A portion of the material in this article was presented before the Division of the History of Chemistry of the American Chemical Society at its meeting in New York City on September 12, 1957.

[1] Archives Researcher, The Franklin Institute.

[2] The boldface numbers in parentheses refer to the references appended to this paper.

A large stack, five stories high, is required for housing the books and sets of periodicals, while numerous works of reference and current journals are available on open shelves in the communicating reading room. Periodicals, bound or current, and works of reference are for use only within the Library. The Library is open to members of the Institute and, for reference, to qualified non-members. It is provided with a microfilm reader, and is prepared to furnish photostats.

The first Committee on Library was appointed on April 1, 1824. Its chairman was Mathew Carey, bookseller and publisher, friend of Franklin and Lafayette. The other members were Samuel Vaughan Merrick, founder of the Institute; William Hypolitus Keating, professor of mineralogy and chemistry in both the University of Pennsylvania and The Franklin Institute; J. Katez, bookbinder; and Peter A. Browne, counsellor. Since then, numerous scientists have served on this Committee, some for periods of three decades or longer. The minutes of the Committee were kept by the Actuary of the Institute until 1875, by the Secretary of the Institute 1875–1902, and by the Librarian thereafter.

During the first half-century of the Institute the Actuary apparently was in charge of the Library. But three persons have held the office of Librarian: Emanuel Hiltebrand, 1874–1887; Alfred Rigling, 1887–1940; and Walter A. R. Pertuch, the present incumbent, since 1941. Thus for seven decades two men have presided over the Library. Each entered the service of the Institute in his early youth. Rigling began his life work in the Library on February 13, 1883 at the age of fourteen and a half years; he became Librarian at the age of nineteen years, and held that office until his death in 1940. Pertuch entered the service of the Library on September 12, 1904 at the age of sixteen years, and succeeded Rigling as Librarian in 1941. The members of the Institute and others who use the Library are deeply indebted to these two gentlemen for their diligent, loyal, devoted, unselfish, highly efficient, and courteous service. Each was elected an Honorary Member of The Franklin Institute upon the completion of fifty years in its service (2,3). Mr. Pertuch will retire as Librarian on April 1, 1960 and become Librarian Emeritus. John P. McGowan became Director of the Library on December 1, 1959.

The first gift to the Library, "sundry copies of the Encyclopaedia plates," was made on April 1, 1824 by Benjamin F. French, an engraver and an original member of the Institute. The first periodicals were presented by Samuel Vaughan Merrick on October 7, 1824; they were sixty-five numbers of four scientific journals "which he had imported from England." The first book was donated by William H. Jones, a merchant and a newly elected member, on January 17, 1825. The first legacy for the Library was the bequest of William S. Warder, a merchant, who became a member of The Franklin Institute in 1824 and served it as a Manager 1825–1828, as Treasurer *pro tem.* in 1825, and

as a member of the Committee on Library in 1825 and 1826. His bequest amounted to twenty-eight dollars and was received in 1832. The first endowment for the Library was the bequest of one thousand dollars by Algernon S. Roberts, a druggist, who became a member of the Institute in 1824, a life member in 1842, and died in 1865. In 1833 the Institute absorbed the Maclurean Lyceum and acquired its collection of books.

During the century and a third of its existence, the Institute has received the corpus of a number of trust funds, the income of which is for the Library, and, in some trusts, is to be used only for certain designated purposes. The Institute also receives the income of other trust funds to be used for designated purposes in the Library. The accompanying table lists these funds, with the year of the creation of each, and the fields to which it applies.

Chemical Journals Binding Fund	1909	Binding chemical journals
Haney Memorial Fund	1959	Additions to Library or collections
Harry W. Jayne Library Fund	1916	Library
M. Carey Lea Bequest	1897	Books and periodicals devoted to chemistry and physics
Henry Leffmann Trust	1931	Books on chemistry and physics
Louis E. Levy Memorial Library Fund	1919	Books on photography, especially on photoreproductive processes
Memorial Library Fund	1884	History of electricity
Willis I. Milham Puzzle Fund	1954	Puzzles
Bloomfield Moore Memorial Fund	1878	Books, their binding and care
James T. Morris Memorial Fund No. 1	1899	Books on mechanics, their binding and care
James T. Morris Memorial Fund No. 2	1932	Books on mechanical subjects, their care and repair; books of reference
Alexander E. Outerbridge Fund	1948	Books on metallurgy
Howard N. Potts Library Bequest	1908	Books
Marion Reilly Memorial Library Fund	1929	Mathematical physics publications
Library Fund in Memory of Alfred Rigling	1947	Bibliographical publications
Algernon S. Roberts Bequest	1866	Books, improvement of Library
Röhm & Haas Company Library Fund	1930	Books and periodicals
Isaac B. Thorn Memorial Library Fund	1913	Books and their care
Elwood Wagner Bequest	1954	Library
Lewis S. Ware Library Fund	1920	Books and periodicals on sugar, and binding of same.

Under certain conditions, the surplus income of certain lecture funds and certain medal funds may be used for library books. These funds, the year of creation of each, and the field in which its excess income may be used are:

Edward G. Budd Lecture Fund	1948	Books
William B. Coleman Lecture Fund	1956	Books on metallurgy or chemistry
Charles Day Lecture Fund	1941	Books
James Mapes Dodge Lecture Foundation	1927	Books
Walton Clark Medal Fund	1926	Books on gas manufacture
Louis E. Levy Medal Fund	1923	Same as Louis E. Levy Memorial Library Fund
John Price Wetherill Memorial Fund	1917	Books

In addition to purchase of books and periodicals from funds appropriated from the general income of the Institute and the income of certain trusts and endowments, the Library has been increased by gifts, bequests, deposits, exchanges of duplicates, and exchanges with the Institute's JOURNAL.

Certain of these gifts and bequests may be mentioned. In 1874 John Lenthall, who became a member of the Institute in 1835 and a life member in 1848, and served as chief of the bureau of construction, United States Navy 1853–1871, presented his collection of books and of original drawings by himself in the field of naval architecture. In 1879 a donation of scientific books from the library of the late Jacob Brown Knight, who became a member of the Institute in 1873 and was its Secretary 1875–1879, was made by his sister, Mrs. E. K. Williams. In 1885 some nine hundred books from the library of Robert Empie Rogers were presented by his sister-in-law, Mrs. William B. Rogers[3] of

[3] Patrick Kerr Rogers, a member of the Chemical Society of Philadelphia and professor of natural philosophy and chemistry in the College of William and Mary (1819–1828), had four sons, at times called the "Brothers Rogers," each of whom attained eminence in physical science. They were: James Blythe Rogers (1802–1852), William Barton Rogers (1804–1882), Henry Darwin Rogers (1808–1866), and Robert Empie Rogers (1813–1884). James B. Rogers was professor of chemistry in the University of Pennsylvania 1847–1852. William B. Rogers was professor of natural philosophy and chemistry in the College of William and Mary 1828–1835, professor of natural philosophy in the University of Virginia 1835–1853, and president of the Massachusetts Institute of Technology 1862–1870 and 1878–1881. Henry D. Rogers was professor of mineralogy and geology in the University of Pennsylvania 1835–1846, and professor of natural history in the University of Glasgow 1857–1866; he was State Geologist in charge of the First Geological Survey of Pennsylvania 1836–1858. Robert E. Rogers was professor of chemistry in the University of Virginia 1842–1852, professor of chemistry in the University of Pennsylvania 1852–1877 and dean of its school of medicine 1856–1877, and professor of medical chemistry and toxicology in the Jefferson Medical College 1877–1884. Three of the brothers were elected to membership in The Franklin Institute, James B. in 1840, Henry D. in 1834, Robert E. in 1838. Upon removal from Philadelphia, Henry D. Rogers

Boston. In 1897 Mathew Carey Lea, who became a member of the Institute in 1847 and a life member in 1848, devised his scientific library to the Institute. In 1901 photographic publications from the library of the late John Sartain were given by his son, Samuel Sartain.[4]

In 1904 Mrs. Mathilde P. Rood and her children presented to the Institute a collection as a memorial to her husband, Ogden Nicholas Rood, who was professor of physics in Columbia University 1863–1902. The collection contains the Poggendorff, Wiedemann, and Drude series (with their supplements) of the *Annalen der Physik und Chemie*, and long runs of the *Fortschritte der Physik* and the *American Journal of Science*.

In 1920 the Institute received the Ware Sugar Library which had been collected and bequeathed by Lewis Sharpe Ware who became a member of the Institute in 1877. He also made a bequest for the care, maintenance and preservation of the Ware collection.

In 1937 a collection on ophthalmology and related subjects from the library of the late L. Webster Fox was presented by his daughter, Beatrice Fox (Mrs. Charles F.) Griffith who had become a life member of the Institute in 1934. Fox was professor of ophthalmology in the Medico-Chirurgical College of Philadelphia 1893–1916 and in the University of Pennsylvania 1916–1931.

In 1951 the library of the National Association of Clock and Watch Collectors was acquired by the Institute. In 1955–1956 The Eberhard Foundation made grants for the purchase of biochemical periodicals. In 1956 the Kline Foundation made a grant for the purchase of chemical treatises.

In 1859 Henry Seybert, who had become a life member of the Institute in 1830, deposited 180 volumes, some of which had belonged to his father, Adam Seybert. Early issues of the *Annales de chimie* are included in the group.

In 1879 by an exchange with Henry Pemberton, a member of the Institute, a set of Liebig's *Annalen der Chemie*, "the only one he believed in Philadelphia," was obtained.

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resigned from the Institute in 1848, Robert E. Rogers in 1843. Upon his return to Philadelphia, Robert E. Rogers was again elected a member in 1852 and became a life member in 1853. Two of the brothers held office in the Institute. Henry D. Rogers was a Manager 1838–1843; Robert E. Rogers was a Manager in 1867 and 1879–1884, Vice-President 1868–1874, and President 1875–1878.

Robert E. Rogers, by his will (recorded at Philadelphia in Will Book No. 116, p. 480) bequeathed his books and other personal property "unto my sister-in-law Emma Rogers widow of my dear brother Professor William B. Rogers to make such distribution of the whole or any portion of the same . . . as she in her sole judgment and discretion may deem advisable and proper."

[4] John Sartain, engraver, became a life member of The Franklin Institute in 1831, and served as a Manager 1877–1879. Samuel Sartain, engraver, became a member in 1861, and a life member in 1863; he was a Manager 1865–1882, Treasurer 1883–1906, and Chairman of the Committee on Science and the Arts 1895–1896.

Among the groups obtained by purchase is one collected by Alexander Dallas Bache at the request of the Institute during his journey, as president of Girard College, to study education in Europe 1836–1838; another group of 58 volumes was purchased in 1895 from the estate of John M. Maisch who was professor of pharmacy (1866–1867) and of materia medica (1867–1893) in the Philadelphia College of Pharmacy.

The Franklin Journal and American Mechanics' Magazine appeared in 1826 "under the Patronage of The Franklin Institute of the State of Pennsylvania." In 1828 a change occurred to the present title, JOURNAL OF THE FRANKLIN INSTITUTE. In addition to functioning as a source of original literature in the field of science and technology, the JOURNAL is of great value in the field of patents as is shown by the following excerpt from its cumulative index (4):

> For a considerable period (1826 to 1859) the *Journal* published a list of the patents granted in the United States. These publications included an abstract of the specifications, and the claim in full, except for the patents of the years 1826–1827 and March to October of 1836, during which time the claims were omitted. In the official publication of patents made by the Government prior to 1843, the claims were omitted, and the *Journal* consequently is the only source at present available for reference to the specifications and claims of patents in the United States during that time. It can also be used in place of the official publications for the entire period (1826–1859) during which the patent lists were published therein.

By exchanges, the JOURNAL has added to the files of periodicals in the Library. The number of journals received by exchange was 27 in 1832, 199 in 1882, and first attained its present value (approximately 400) in 1893.

Included in the Library are certain publications of the Institute, now out of print, which are of deep interest to the student of the history and progress of American science and technology. These publications are the catalogues and/or reports of its exhibitions and the record of the awards of its medals. The Institute conducted twenty-six exhibitions of American manufactures between 1824 and 1858, also one in 1874. In 1884 it conducted the International Electrical Exhibition, in 1885 the Novelties Exhibition, and in 1899 was co-sponsor of the National Export Exposition.

A list of the medal awards of the Institute with the reason for each award was published in 1958 (5).

Among the rare books in the Library is a group of those printed by Benjamin Franklin, over sixty in number (6); among them is Thomas Cadwalader's "Essay on West-India Dry-Gripes" published in 1745, of interest to the student of lead poisoning. The Library also owns the

account book of Richard Bache (7) which includes sundry accounts with his father-in-law, Benjamin Franklin.

A bound volume, once the property of Adam Seybert and of Henry Seybert, contains fourteen theses presented for the degree of Doctor of Medicine and printed in the period 1786–1793; thirteen were presented at the institution now known as the University of Pennsylvania, among them are those of John Penington, James Woodhouse, and Adam Seybert, pioneer American biochemists.

Among the chemical books published before 1700 are two treatises by Paracelsus, two by Glauber, and four by Agricola. The Library also has the translation of Agricola's "De Re Metallica" made by Herbert Clark Hoover and Lou Henry Hoover and published at London in 1912. A list of "Some Early Classics of Science and Technology" in the Library (8) issued in 1954 describes sixty-five treatises, each published prior to 1700. Other rare works in the Library include those on sun dials and astrolabes, the gift of Harrold E. Gillingham; and those on astronomy and related subjects, the gift of Gustavus W. Cook.

The Library has, on microfilm, all available United States Patents from 1790 to 1835, both inclusive, and, in printed form, a complete set of these patents beginning in 1871. It possesses complete specifications of British Patents beginning in 1617, and Swiss Patents beginning in 1888, also Japanese Patents 1909–1941, abstracts of German Patents 1877–1940 and since 1950, and complete sets of abstracts of Canadian Patents and Australian Patents.

The Library is included in "A List of Serials In the Principle Libraries of Philadelphia and its Vicinity" published in 1908 with a Supplement in 1910 (9). It is also included in the card index "Union Library Catalogue of the Philadelphia Metropolitan Area," and is a cooperating library in the "Union List of Serials in Libraries of the United States and Canada" and also in the "List of Periodicals Abstracted," which is published periodically by *Chemical Abstracts*.

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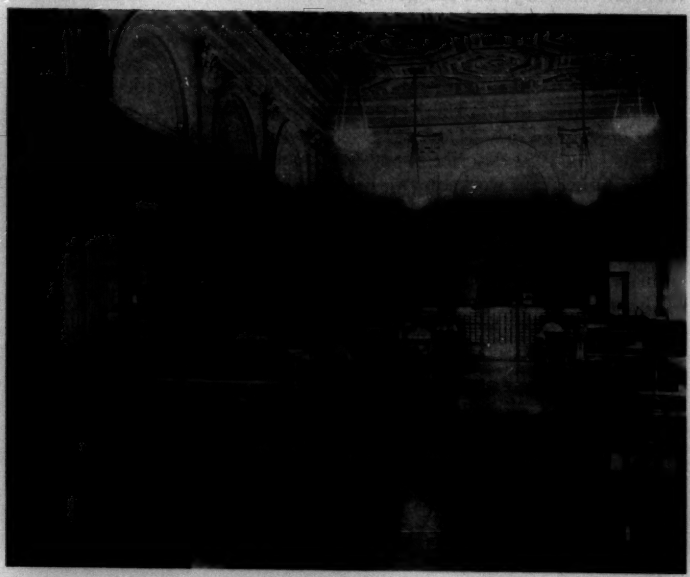

Pepper Hall, the Reading Room of The Franklin Institute Library.

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THE FRANKLIN INSTITUTE

MINUTES OF THE STATED MEETING

February 17, 1960

The Stated Meeting of The Franklin Institute was held at 8:15 P.M. in the Lecture Hall. Mr. Wynn Laurence LePage, President, called the meeting to order with approximately 250 members and guests present.

The President stated that the minutes of the Stated Meeting of December 16, 1959 were published in the January issue of the JOURNAL. There being no additions or corrections, the minutes were approved as published. He then announced that the minutes of the Annual Meeting of January 20, 1960 will be published in the next issue of the JOURNAL and will be presented at a later Stated Meeting of The Institute for approval.

The President noted that this was the occasion of the Fifth Annual Philip C. Staples Lecture. This lecture, in honor of Mr. Staples who had been President of The Franklin Institute and President of the Bell Telephone Company of Pennsylvania, was established by friends of Mr. Staples as a memorial to him. And the President continued by saying that we were proud to have Mrs. Staples with us this evening, who continues to have great interest in our activities.

Mr. LePage then introduced the speaker of the evening, The Honorable Warren G. Magnuson, United States Senator from Washington, whose subject was "Research in the Marine Sciences." Senator Magnuson brought out the importance of marine science to our contemporary way of life, and how vital this science is to our defense. He brought out that oceanography encompasses studies of the ocean floor, radioactive wastes in ocean water, extraction of metals from beneath the seas, and diseases of marine life. Oceanographers are using bathyscaphs, icebreakers, telemetering equipment, echo sounders and deep sea cameras.

The President expressed on behalf of The Institute our grateful appreciation to Senator Magnuson for coming to us with this important message.

The meeting was adjourned at 9:35 P.M.

WILLIAM F. JACKSON, JR.
Secretary

MEMBERSHIP

1959 SILVER MEMBERS HONORED

At the Stated Meeting on December 16, 1959, the Institute honored 45 of its members who had completed 25 consecutive years of membership. Prior to the meeting, the nine new Silver Members who were able to attend were guests of honor at a dinner and reception. During dinner, Wynn Laurence LePage, President of the Institute, introduced these honored guests: Leighton F. Appleman, Herbert Church, Norbert A. Considine, William J. Kassimir, M. Albert Linton, C. H. MacDonald, Arthur Neumann, Jr., Horace Pettit and Charles Gormley Stehle. As souvenirs of the occasion, the new Silver Members received initialled paper weights containing 1959 quarters.

Morton Gibbons-Neff (right), Chairman of the Institute's Membership Committee, extends congratulations to eight of the 1959 Silver Members who were honored at the dinner on December 16. They are (l. to r.): seated, Herbert Church, William J. Kassimir, M. Albert Linton and Leighton F. Appleman; standing, C. H. MacDonald, Charles Gormley Stehle, Horace Pettit and Norbert A. Considine. Arthur Neumann, Jr., also a 1959 Silver Member, arrived too late for the picture.

V.2
196

NEW MEMBERS

The following elections were approved on February 4, 1960 by the Membership Committee by authority given to it by the Board of Managers.

ANNUAL

Keeton Arnett
Louis H. Averbach
Walter D. Banes
Thomas J. Berry
William Berry
Frederick E. Blum
Murray C. Boyer
Mrs. Newell C. Bradley
Albert I. Bryant
Peter R. Buechler
Fred H. Coats
Carl S. Dash
Harry Davis
Gertrude Ely
Raymond P. Farrell
Howell K. Fesq
John E. Geverd
George J. Glick
Richard W. Goudie
Roy J. Graeter
Richard J. Grimm
Frederick G. Hall, II
Harry H. Heins
Mrs. T. Charlton Henry
Talmadge W. Hopson
Lee Hrynick
Samuel Katz
Jack E. Kindregan
W. S. Kirkpatrick
Edwin L. Layton, Jr.
Donald R. Lenton
Donald F. Manger
Peter P. Muessig, Jr.
W. J. Murray
Richard L. Newburger
Robert D. Neyer
Garrett D. Pagon
M. C. Rainone
Harold Rapaport
Robert J. Renfrow
George H. Rhodes
Walter C. Rieker
Jerome David Sable
John B. Sangree
Francis A. Schanne
Alvin J. Scheel
Philip E. Scott
Peter H. Sellers
Seymour W. Shapiro
Robert A. Sheeran
Bruce M. Smith
Elmer R. Swift
Harry H. Verdier
Richard M. Walsh
Lawrence P. White
Herbert Zahner
Jean J. Zepke
Eugene J. Zurbach, Jr.

EDUCATOR

George H. Auth
Zachary S. Bernstein
Edwin M. Chance, II
A. E. Richard deJonge
Thomas M. Earnshaw
Walter Edmiston
Roger C. Heimer
Walter Van Ness Pruyn
Clifford E. Seglem
Mrs. H. A. Walens

NECROLOGY

John C. Cornelius, Jr. '15
G. Barrett Glover '58
Otto Haas '36
George A. Huggins '42
Heinrich Kaffine '45
Friedrich G. Krantz '58
R. W. Myers '45
Otto F. Schoenhut '55
Thomas M. Scott '36
H. Birchard Taylor '23
H. Winfield Wright '43

ROLL OF HONOR

The following are Gold Card Members, having belonged continuously for fifty years or more. The dates are years of election to membership.

Fred H. Colvin '88
G. H. Clamer '91
J. H. Granberry '95
Edward Woolman '95
E. A. Muller '99
James M. Caird '01
Charles H. Howson '03
Frank S. Busser '04
C. Mahlon Kline '04
George R. Hall '05
Darthela Clark '07
Frank Shaw Clark '07
W. M. C. Kimber '08
Theobald F. Clark '09
Walton Clark, Sr. '09
James McGowan, Jr. '09
T. Edward Ross '09
Richard P. Brown '10
Joseph S. Hepburn '10
Joseph W. Lippincott '10
J. D. Shattuck '10
J. M. Weiss '10

The following are Silver Members, having belonged continuously for twenty-five years or more. The dates are years of election to membership.

1912

James Barnes
Morris Llewellyn Cooke
Edwin Elliot
Walter Palmer
Julian S. Simsohn
Howard S. Worrell

1913

Irenee duPont
S. B. Eckert
Ralph E. Flanders
N. E. Funk
Lionel F. Levy
E. Mallinckrodt, Jr.
Frank M. Masters
Franklin P. McConnell
William Maul Measey
W. Chattin Wetherill
D. Robert Yarnall

1914

George S. Crampton
Ernest L. Huff
K. G. Mackenzie
George Wharton Pepper

1915

Thomas D. Cope
H. Jermain Creighton
Thomas W. Elkinton
Richard Howson
W. Wallace McKaig

1916

Henry B. Allen
James G. Detwiler
Zay Jeffries
Frank H. Sauer
B. E. Shackelford
Arthur Synnestvedt
Charles N. Weyl

1917

A. D. Chambers
Francis J. Chesterman
Walter S. Crowell
William J. Fitzmaurice, Jr.
Rolfe E. Glover, Jr.

1917 (cont.)

Arthur W. Lowe
Frank S. MacGregor
Francis F. Milne, Jr.
C. H. Quinn
Sylvan D. Rolle
Robert L. Wood
Henry Woodhouse

1918

Charles H. Colvin
Walter O. Snelling

1919

Charles E. Brinley
Henry Colvin, 2nd
E. A. Eckhardt
George W. Furness
Frank H. Griffin
J. Howard Pew
Edmund G. Robinson
L. K. Sillcox

1920

Chester Lichtenberg
P. S. Lyon
Haviland H. Platt
Carl D. Pratt

1921

Roland L. Andreau
Leonard T. Beale
J. Ed. Brewer
Arthur L. Day
Frederic Palmer, Jr.
Howard Stoertz
James Stokley
Edward R. Weidlein
S. Weinberg

1922

Joseph F. Greene
C. E. K. Mees

1923

Henry Clay Gibson
Howard S. Levy
William C. Melcher, Jr.

1923 (cont.)

John F. Metten
Richard G. Sagebeer
Joseph J. Vogdes

1924

Peter Abrams
T. R. Harrison
Valentine Hiergesell
Karl F. Oerlein
Edward B. Patterson
Nicholas G. Roosevelt
Alfred O. Tate
James Lloyd Weatherwax
Alexander Wilson, 3rd
William Zimmermann

1925

Jack G. Binswanger
Marion Eppley
John Graham Foley
J. V. Giesler
C. R. Kraus
H. F. Porter
W. F. G. Swann

1926

Charles B. Bazzoni
Niels Bohr
William Findlay Downs
Henry C. Evans
Edward L. Forstall
Lewis H. Hendrixson
Charles S. Leopold
Alfred L. Loomis
J. H. Manning
Clement B. Newbold
O. M. Patton
Dexter N. Shaw
Sydney L. Wright
Charles S. Wurtz, Jr.

1927

T. K. Cleveland
William DeKrafft
Hiram B. Ely
Leslie Griscom
Alfred Iddles
John Van Gasken Postles

1927 (cont.)

Philip Sporn
Charles A. Stanwick
Charles A. Young

1928

William R. Dohan
Walton Forstall, Jr.
C. W. Hansell
Walter A. MacNair
Fred P. Nabenhauer
Joseph N. Pew, Jr.
William Rogers, Jr.
Frank N. Speller

1929

N. E. Bonn
Bernard Davis
Rupen Eksergian
Edward C. Haines
Malcolm Lloyd Hayward
Linn Helander
Randal Morgan
Harold F. Pitcairn
Lawrence Saunders
A. Kelvin Smith
I. Melville Stein
William Stericker
Harden Franklin Taylor
Jacques L. Vauclain
Albert J. Williams, Jr.
Arthur M. Wilson, Jr.
Enos E. Witmer

1930

Mildred Allen
William M. Barret
Aldus Fogelsanger
John Gardiner, Jr.
Morton Gibbons-Neff
Norman R. Gibson
Carroll Hodge
Walter J. Lehman

1930 (cont.)

R. P. Long
S. B. Morehouse
William C. Peterman
G. E. Wiltbank

1931

Charles E. Berger
Carl Breer
Abraham D. Caesar
Daniel B. Curll, Jr.
Edward Dawson
J. Bennett Hill
Frederick C. Holtz
Penrose R. Hoopes
William Melas
Perry H. Moon
Julius Neumueller
Laurence P. Sharples

1932

Arthur Fleischer
William A. Heine
Wynn Laurence LePage
Robert McLean
E. Burke Wilford

1933

Charles A. Bareuther
George E. Crofoot
George S. Gardner
Walter H. Hartung
Karl S. Howard
Edward R. Hughes
Annetta R. Masland
Ellen-Jarden Nolde
J. Livingstone Poultney
George A. Smith
Floyd T. Tyson
Wilfred H. Wickersham

1934

Add B. Anderson
Leighton F. Appleman

1934 (cont.)

Eleanor Bartol
Thomas Campbell Bagg
Charles G. Berwind
Henry W. Breyer
Reynolds D. Brown, Jr.
Edward Browning
Orville H. Bullitt
Herbert Church
Norbert A. Considine
Jay Cooke
Joseph Curry
Charles D. Dickey
Harold Evans
Philo Taylor Farnsworth
Mrs. Charles Fearon
Kermit Fischer
Mrs. S. K. Fuller
Mrs. William H. Greene
Mrs. Charles F. Griffith
J. G. Richard Heckscher
Russell P. Heuer
Walter M. Jeffords
William J. Kassimir
M. Albert Linton
H. Gates Lloyd
C. Townsend Ludington
C. H. MacDonald
Orus J. Matthews
Arthur H. Nellen
Arthur Neumann, Jr.
Horace Pettit
Edgar Allan Poe
B. Brannan Reath, 2nd
Stanley C. Snow
Julian F. Spiegel
Charles Gormley Stehle
Harry C. Tily, 2nd
Mrs. R. Marshall Truitt
Clarence A. Warden, Jr.
Paul T. Warner
Mrs. William White
Miss Marion B. Wood
Mrs. Efrem Zimbalist

COMMITTEE ON SCIENCE AND THE ARTS

(Abstract of Proceedings of Stated Meeting held Wednesday, February 10, 1960.)

HALL OF THE COMMITTEE,
PHILADELPHIA, FEBRUARY 10, 1960.

MR. J. PHILIP EVANS in the Chair.

The following report was presented for final action:

No. 3351: Amperometric Measurement of Water Vapor in a Flowing Gas.

This report recommended the award of an Edward Longstreth Medal to Frederick A. Keidel, of Wilmington, Delaware, "For his development of a novel continuous electrolytic moisture analyzer, now used in large quantities throughout the country."

D. S. FAHRNEY,
Secretary to Committee

BARTOL RESEARCH FOUNDATION
MARTIN A. POMERANTZ, DIRECTOR

BALLOON FLIGHT INVESTIGATIONS OF PRIMARY COSMIC RAYS DURING SOLAR DISTURBANCES *

BY

M. A. POMERANTZ, S. P. AGARWAL AND V. R. POTNIS

ABSTRACT

An extensive series of balloon flights was conducted during the peak of the present solar maximum, utilizing instruments identical with those which had been flown previously during the declining portion of the preceding solar cycle (1949–1952). During the interval July, 1957, to September, 1958, no sporadic increases in intensity were detected at Swarthmore, Pennsylvania, by quadruple coincidence counter trains containing 86 g/cm² of absorber. In eight cases, solar flares of Importance 2 or greater occurred while the instruments were aloft, and on one occasion (22 Aug. 1958) a large increase in the flux of low energy cosmic rays was observed at balloon altitudes elsewhere. A recurrence tendency with a periodicity of 28 ± 1 days was followed for about 5 months. The fluctuations in the counting rates of the balloon-borne instruments were correlated with those observed by ground-based neutron monitors, and the mean factor relating the relative changes is 1.6 ± 0.3. However, it is unlikely that this ratio remains constant.

I. INTRODUCTION

Following the first observation by means of a balloon-borne instrument of an increase in the primary cosmic ray flux specifically associated with an outstanding solar flare (1)[1], a program of flights was conducted in 1949–1952 during the declining portion of the preceding solar cycle in an attempt to detect additional events in which cosmic rays arrive directly from the sun. A warning system similar in principle to that later established for the International Geophysical Year was set up on a very modest scale, and balloon flights were launched during periods which corresponded in many respects to the Special World Intervals of IGY.

A complete report of the results of the flights which were aloft coincident with the occurrence of Hα flares, either on the basis of predictions of impending solar events, or by chance during the course of other investigations, was presented at the Guanajuato Conference (2). Several examples of the enhancement of the primary cosmic ray flux by solar-emitted particles capable of penetrating appreciable thicknesses of absorber (as much as 86 g/cm² plus the residual atmosphere) were detected by the balloon-borne instruments. Recently, similar increases have been observed at high altitudes by other investigators (3). The earlier search for sporadic increases in the intensity of low energy pri-

* Assisted by the U.S. National Committee for the IGY, and by the Office of Naval Research. Presented at the International Cosmic Ray Conference, International Union of Pure and Applied Physics, Moscow, July 6–11, 1959.

[1] The boldface numbers in parentheses refer to the references appended to this paper.

TABLE I.—*Balloon Flights at λ = 52° N and Solar Events During IGY.*

Flight Date	Hα-Flares				2800 Mc./Sec. Radio Events				Primary Cosmic Radiation	
	Importance	Time Observed, UT	Time of Max., UT	Position, Degrees	CRPL Classification	Time Observed, UT	Time of Max., UT	Peak Flux, 10^{-22} w./m.^{2} (c./s.)	Residual Atmosphere at Ceiling, g./cm.$^{-2}$	Time of Arrival at Ceiling, UT
17 July '57	1	1021–1036		N10 E15	Simple 2A	1219–1221	1220	72	5	1318
	2	1218–1235	1220	S36 E20	Simple 2	1220–1221	1220	60		
	1	1303–1347		S21 E56						
2 Sept. '57	2+	1046–1610		S30 W33	3 Simple 3fA	1247–1657	1330	105	9	1700
	2	1259–1335		N10 W26	2 Simple 2	1258–1304	1300	56		
	2	1313–1408	1316	S32 W36	8 Group 2	1317–1327		30		
	3	1338–1830	1425	S35 W36	2 Simple 2	1631–1633	1632	14		
	2	1346–1507		S34 W37	6 Complex	1810–1816	1814	9		
	1	1415–1505	1427	N12 W28	[8 Group 2	2058–2110		16		
	1	1509–1513		N20 W17	6 Complex]					
	1	1600–1611		N20 W26						
	1	1810–1858	1812	N14 W36						
	1	2100–2112	2101	S28 W30						
	1	2130–2300	2147	S32 W43						
4 Sept. '57	2	1156–1236		N15 W54	3 Simple 3fA	1156–1621	1255	18	25	1514
	1+	1200–1246		N12 W52	6 Complex	1158–1214	1200	22		
					2 Simple 2	1243–1250	1245	14		
12 Sept. '57	1	1130–1143	1135	S18 W12	2 Simple 2	1238–1242	1239	13	8	1700
	1+	1213–1217		S18 W07	2 Simple 2f	1514–1532	1515	850		
	1+	1409–1422		N10 W17						
	1	1444–1451	1448	S17 W15						
	2+	1510–1550	1517	N11 W19						
	3	1514–1536	1516	N10 W18						
	2	1517–1638		N11 W18						
	1	1535–1622	1543	S16 W15						
	1	1632–1642	1635	S16 W15						
	1	1657–1705		S14 W15						

TABLE I.—*Continued*

	Hα-Flares				2800 Mc./Sec. Radio Events				Primary Cosmic Radiation	
Flight Date	Importance	Time Observed, UT	Time of Max., UT	Position, Degrees	CPRL Classification	Time Observed, UT	Time of Max., UT	Peak Flux, 10^{-22} w./m.^{2} (c./s.)	Residual Atmosphere at Ceiling, g./cm.$^{-2}$	Time of Arrival at Ceiling, UT
29 Oct. '57	1	1215–1225		N21 W48	2 Simple 2f	1514–1517	1516	10	6	1517
	1	1307–1341	1320	N26 E12	3 Simple 3	1532–1632	1540	17		
	1	1515–1530		N23 W66						
	2	1515–1525	1519	N12 E71						
	1	1518–1530		N14 W72						
	1	1531–1553	1540	N22 W28						
	1	1534–1551	1541	S20 E32						
1 May '58	2	1806–1905	1817	S16 E28	3 Simple 3A	1745–2015	Indet.	13		
	1+	1910–1945	1914	S18 E12					6	2125
	2+	2046–2120	2109	S18 E15						
	3	2115–2228	2135	S20 E20	3 Simple 3A	2102–2117	2107	9		
2 May '58	1	1140–1155	1144	S20 E08					7	1401
	1	1205–1222		S17 E09						
	1	1305–1309		N31 W14						
	2	1305–1325		N24 E07						
	1	1335–1350	1340	N31 W36						
	1	1413–1430		N30 W35						
22 Aug. '58	3	1417–1717	1506	N21 W08	3 Simple 3fA	1305–1835	Indet.	45	7	1713
	1	1728–1733		N26 W77	6 Complex	1320–1328	1323	13		
	1	1819–1957		N13 W16	2 Simple 2f	1430–1630	1506	1500		

maries was discontinued in 1952, but was resumed during IGY. The later results will be presented here.

It became apparent during the initial phase of these experiments that the intensity at high altitudes was quite variable over short periods of time, in contrast with the situation which prevailed during the earlier series of flights. During 1947–1952, except for the flare-associated increases, the flux at the "top" of the atmosphere, as measured with coincidence counter trains containing interposed absorber, had remained

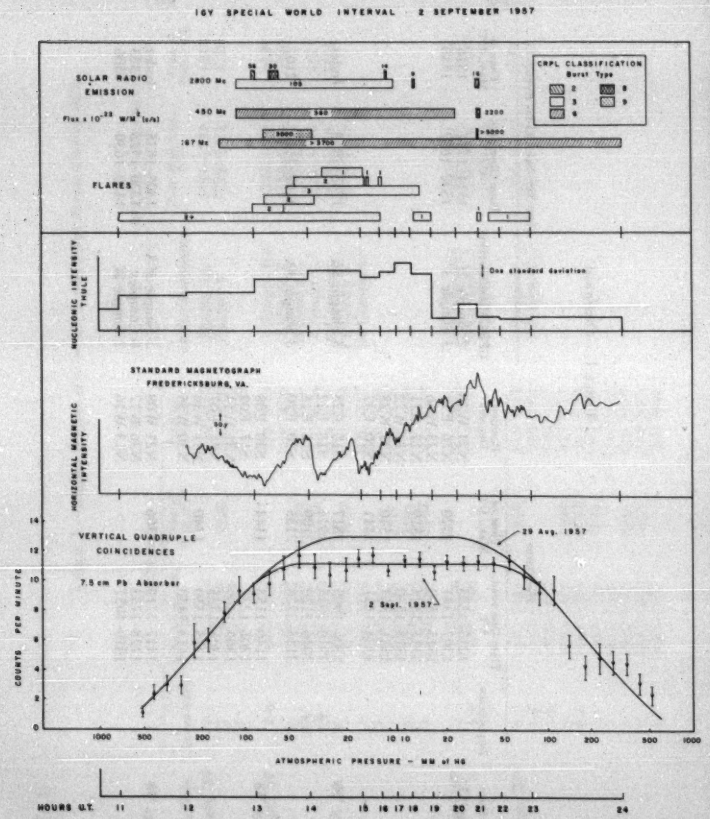

FIG. 1. Cosmic ray observations and relevant geophysical data obtained during local daylight hours on 2 September 1958. Despite the exceedingly high level of solar activity, no increase in the intensity of particles capable of penetrating 7.5 cm of Pb was detected near the top of the atmosphere at Swarthmore.

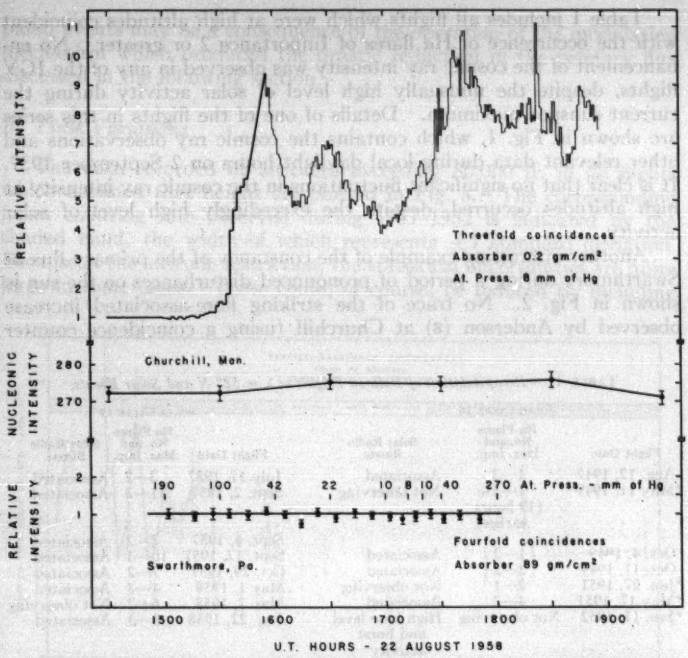
RELATIVE INTENSITY
Threefold coincidences
Absorber 0.2 gm/cm²
At. Press. 8 mm of Hg
NUCLEONIC INTENSITY
Churchill, Man.
RELATIVE INTENSITY
190 100 42 22 10 8 10 40 270 At. Press. - mm of Hg
Fourfold coincidences
Absorber 89 gm/cm²
Swarthmore, Pa.
1500 1600 1700 1800 1900
U.T. HOURS - 22 AUGUST 1958

FIG. 2. Data obtained with balloon-borne instrument at Swarthmore, Pennsylvania during period of unusual intensity increase observed by Anderson at Ft. Churchill. Neutron monitor data obtained at Churchill during this period supplied by courtesy of Dr. D. C. Rose.

constant from flight to flight, within the statistical uncertainties (although Neher (4) frequently observed fluctuations at high altitudes with thin-walled ionization chambers). Consequently, a series of flights was conducted in an effort to follow the variation, and a 27-day recurrence tendency was noted (5, 6). The same phenomenon was independently observed by Vernov *et al.* (7). Since the results of the balloon flight program at Swarthmore have been published only in preliminary form, the present paper will include all observations of the intensity at high altitudes at this station during IGY.

II. RESULTS

A. Flights During Solar Flares

The apparatus was identical with that utilized previously (1). In all flights in this series, the four-fold coincidence counter train contained 86 g/cm² of interposed lead (instrumental cutoff for protons 0.27 Bev).

Table I includes all flights which were at high altitudes coincident with the occurrence of Hα flares of Importance 2 or greater. No enhancement of the cosmic ray intensity was observed in any of the IGY flights, despite the unusually high level of solar activity during the current sunspot maximum. Details of one of the flights in this series are shown in Fig. 1, which contains the cosmic ray observations and other relevant data during local daylight hours on 2 September 1957. It is clear that no significant fluctuations in the cosmic ray intensity at high altitudes occurred, despite the exceedingly high level of solar activity.

Another interesting example of the constancy of the primary flux at Swarthmore during a period of pronounced disturbances on the sun is shown in Fig. 2. No trace of the striking flare-associated increase observed by Anderson (8) at Churchill (using a coincidence counter

TABLE II.—*Recapitulation of Balloon Flights at λ = 52° N and Solar Events.*

Solar Cycle 18			Solar Cycle 19		
Flight Date	Hα Flares No. and Max. Imp.	Solar Radio Bursts	Flight Date	Hα Flares No. and Max. Imp.	Solar Radio Bursts
Aug. 12, 1947	1—3	Associated	July 17, 1957	3—2	Associated
*May 11, 1949	1—3+ (19 hours earlier)	Not observing	Sept. 2, 1957	11—3	Associated
			Sept. 4, 1957	2—2	Associated
Oct. 4, 1949	1—2	Associated	Sept. 12, 1957	10—3	Associated
Oct. 11, 1949	2—2	Associated	Oct. 29, 1957	7—2	Associated
*Jan. 27, 1951	2—1	Not observing	May 1, 1958	4—3	Associated
*May 17, 1951	4—2	Associated	May 2, 1958	6—2	Not observing
*Jan. 17, 1952	Not observing	High base level and burst activity	Aug. 22, 1958	3—3	Associated

* Flights with enhanced cosmic-ray flux.

train containing considerably less absorber) was detectable at Swarthmore. This is a consequence of the steepness of the spectrum of the solar protons in this event. No effect at sea level was indicated even by the Churchill neutron moniter.

Table II is a condensed summary of the flights during which solar events occurred in both the earlier and present series. Despite the relatively small sampling, the negative result in the latter case may be of some significance, and perhaps is not merely fortuitous. From an analysis of the Climax neutron monitor data for the period May, 1951, to July, 1952, Firor (9) concluded that an increase of about 1 per cent was associated with flares when the detector was in the morning impact zone. Towle and Lockwood (10) analyzed the Mt. Washington neutron monitor data for the period 1956 and 1957 in a similar manner, and found no evidence for a flare-associated increase $\geq$ 0.25 per cent even when the detector was in the impact zone. The dearth of the antici-

pated events may be a consequence of the operation of the modulation mechanism which produced the drastic reduction in the primary flux discussed in the following section.

B. Time Variations

The data recorded at altitudes exceeding 80,000 ft. in all flights which attained that height are plotted in Fig. 3. The corresponding mean counting rate observed during 1947–1952 is indicated by the shaded band, the width of which represents ± 1 standard deviation. Because of the altitude restriction, the statistical uncertainties are quite large in some cases. These could, of course, be reduced by including

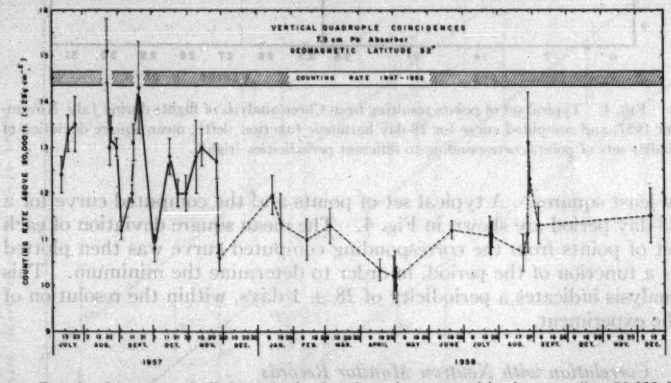

FIG. 3. Summary of all data obtained at Swarthmore at altitudes exceeding 80,000 ft. during the period July 1957–December 1958. The corresponding mean counting rate during 1947–1952 is indicated by the shaded band.

data from lower altitudes, but for the present analysis, attention has been confined to the region of the intensity *vs* altitude curves in which the counting rate of the present apparatus is invariably constant (6).

During most of the IGY, the primary cosmic ray flux was well below the normal value established during the previous solar maximum, and confirmed by pre-IGY flights in December, 1956.

A Chree analysis was performed upon all of the points joined by the solid line in Fig. 3, since the distribution of the flights was adequate for this purpose only from July through November, 1957. In order to determine the period, recurrence tendencies from 25 to 31 days were successively assumed. In each case, the harmonic function of the form $y = A + B \sin(\theta + \delta)$ yielding best fit was calculated by the method

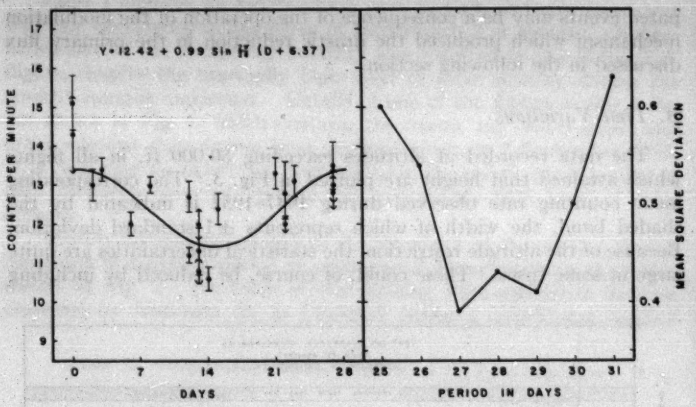

FIG. 4. Typical set of points resulting from Chree analysis of flights during July–November 1957, and computed curve for 28-day harmonic function (left); mean square deviation of similar sets of points corresponding to different periodicities (right).

of least squares. A typical set of points and the computed curve for a 28-day period are shown in Fig. 4. The mean square deviation of each set of points from the corresponding computed curve was then plotted as a function of the period, in order to determine the minimum. This analysis indicates a periodicity of 28 ± 1 days, within the resolution of the experiment.

C. Correlation with Neutron Monitor Records

Qualitatively, the fluctuations in the counting rates of the balloon-borne instruments are positively correlated with those observed by ground-based neutron monitors. In Fig. 5, the counting rate above 80,000 ft. at Swarthmore is plotted against the nucleonic intensity at Ottawa.[2] The points are numbered consecutively, and the flight dates are listed in Table III. It is clear from the scatter of the points that the proportionality factor is far from constant during the entire period of the observations. However, on the basis of the quite arbitrary assumption that the nucleonic intensity is directly proportional to the total primary flux, a straight line was computed by the method of least squares. The dashed lines in Fig. 5 represent one standard deviation. From the slope the ratio of the balloon flight (BF) and neutron monitor (NM) data, $(\Delta I/I)_{BF}/(\Delta I/I)_{NM} = 1.6 \pm 0.3$. The balloon-borne in-

[2] We wish to thank Dr. D. C. Rose for supplying the neutron monitor data from the Canadian stations.

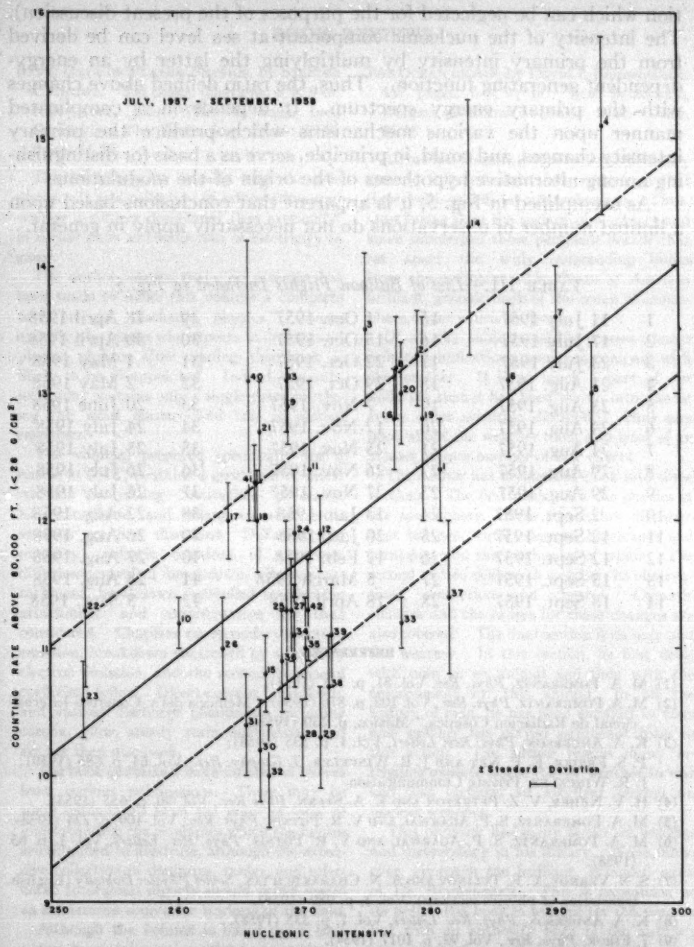

FIG. 5. Counting rate above 80,000 ft. at Swarthmore plotted as a function of nucleonic intensity. Neutron monitor data from Ottawa supplied through the courtesy of Dr. D. C. Rose. The flight dates corresponding to the consecutive numbers are listed in Table III.

strument measures the intensity of primaries (plus an albedo contribution which can be neglected for the purposes of the present discussion). The intensity of the nucleonic component at sea level can be derived from the primary intensity by multiplying the latter by an energy-dependent generating function. Thus, the ratio defined above changes with the primary energy spectrum. It depends in a complicated manner upon the various mechanisms which produce the primary intensity changes, and could, in principle, serve as a basis for distinguishing among alternative hypotheses of the origin of the modulations.

As exemplified in Fig. 5, it is apparent that conclusions based upon a limited number of observations do not necessarily apply in general.

TABLE III.—*List of Balloon Flights Included in Fig. 5.*

1	11 July 1957	15	1 Oct. 1957	29	18 April 1958
2	17 July 1957	16	15 Oct. 1957	30	30 April 1958
3	23 July 1957	17	22 Oct. 1957	31	1 May 1958
4	21 Aug. 1957	18	29 Oct. 1957	32	2 May 1958
5	23 Aug. 1957	19	7 Nov. 1957	33	20 June 1958
6	24 Aug. 1957	20	12 Nov. 1957	34	24 July 1958
7	24 Aug. 1957	21	25 Nov. 1957	35	25 July 1958
8	29 Aug. 1957	22	26 Nov. 1957	36	26 July 1958
9	29 Aug. 1957	23	27 Nov. 1957	37	26 July 1958
10	2 Sept. 1957	24	13 Jan. 1958	38	22 Aug. 1958
11	12 Sept. 1957	25	20 Jan. 1958	39	26 Aug. 1958
12	12 Sept. 1957	26	12 Feb. 1958	40	27 Aug. 1958
13	13 Sept. 1957	27	5 March 1958	41	28 Aug. 1958
14	18 Sept. 1957	28	18 April 1958	42	5 Sept. 1958

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BOOK REVIEWS

BASIC DATA OF PLASMA PHYSICS, by Sanborn C. Brown. 336 pages, diagrams, 6 × 9 in. New York, John Wiley & Sons, Inc., Cambridge, The Technology Press; 1959. Price, $6.50.

This book is quite literally what the title states. It is a collection of information concerning a subject designated (less exotically) in earlier days as conduction of electricity in gases.

The author asserts that "no attempt has been made to make this volume a complete text on gas discharge physics." Consequently, the reader who expects to understand plasma physics after reading this book is likely to be disappointed. Indeed, the subject index contains only a single entry involving the word *plasma*—and this is *plasma resonance*.

The volume, prepared specifically for a course at MIT, contains a good deal of information concerning ionization processes. Elastic-collision and charge-transfer cross sections are first discussed. Diffusion and mobility, inelastic collisions of electrons, diffusion-controlled breakdown, electron attachment, breakdown controlled by electron attachment, and recombination are then considered. Chapters on secondary electron emission, breakdown controlled by secondary electron emission, and the second Townsend coefficient follow. Direct-current breakdown, and various discharge phenomena including corona, glow, steady state microwave, and arc are then discussed.

The book contains a large number of curves from various publications. There may be some redundancy from the point of view of understanding the principles which the figures are intended to illustrate, although the extensive collection of diagrams, tables, and references to original reports should prove useful as a reference source for workers in the field.

Although the volume is based upon class notes, it contains no problems, an omission which might tend to limit its acceptance as a textbook.

MARTIN A. POMERANTZ
Bartol Research Foundation

THE OCEAN OF AIR, by David I. Blumenstock. 457 pages, 6 × 9¼ in. New Brunswick, Rutgers University Press, 1959. Price, $6.75.

All too few authors will devote the necessary time to fully research their subject so that their writings will be comprehensive. Thus, most books skim the surface of a subject and leave submerged those pertinent details that set apart the truly outstanding books from the ordinary. *The Ocean of Air* is a brilliant, graphic book of the ocean of atmosphere under which we live.

Of all the subjects which possess almost infinite ramifications, none can compare with the weather. It is so much a part of our daily life that it has been woven into almost every other scientific discipline. Thus any book about the weather must also treat of an almost infinite number of side effects.

The author has divided his book into three sections. The first deals with the physics of the atmosphere. Here the author discusses heat balance, water, deserts, hurricanes and tornadoes and the mechanics of winds. The second section deals with weather; its observations, prediction and control. Climatic changes and the causes for these changes are also covered. The final section is on man and the weather. In this section, he first deals with man as an animal and then with the environment for this animal. In all the history of man, he has been engaged in wars and weather has played significant roles in this action. The author relates many intriguing aspects of the role of weather in war and also includes an excellent section on nuclear weapons.

This reviewer has many books on weather and meteorology in his library. None, however, approach the brilliant, balanced work of Dr. Blumenstock. This is exposition at its best. His book is written for the layman but with an authority born of the author's close association with leaders in the various scientific disciplines which he explores.

I. M. LEVITT
The Fels Planetarium

BOOK NOTES

THE CHALLENGE OF CHEMISTRY, by O. A. Battista. 168 pages, illustrations, 6 × 9 in. Philadelphia, The John C. Winston Company, 1959. Price, $3.50.

Written for the layman by a research chemist, this book deals with the challenging roles open to young chemists in these fields of chemistry—organic, inorganic, physical, analytical, agricultural and food, high polymer, biochemistry, medicinal, radiochemistry, industrial and engineering chemistry, and teaching. The book is written in a remarkably readable style and is full of little-known historical events in chemistry. The author closes the book with two chapters dealing with the moral and the technological challenges of the future of chemistry. School libraries would do well to stock this book for use in career-planning conferences.

TELEMETERING SYSTEMS, by Perry A. Borden and W. J. Mayo-Wells. 349 pages, illustrations, 6 × 9 in. New York, Reinhold Publishing Corp., 1959. Price, $8.50.

Both stationary and mobile telemetering are covered in this book, thus greatly extending the coverage over Borden's previous "Principles and Methods of Telemetering." In this almost completely revised edition of the older work, the material is practically all new, but the approach is the same—to study types and principles of telemetering rather than specific commercially available apparatus. Fourteen chapters deal with various systems of telemetering (current, voltage, frequency, position and impulse), with pick-ups, links and channels, and with the applications of the different systems.

PHYSICS AND GEOLOGY, by J. A. Jacobs, R. D. Russell and J. Tuzo Wilson. 424 pages, diagrams, 6 × 9 in. New York, McGraw-Hill Book Co., Inc., 1959. Price, $9.75.

In this volume the authors have a twofold purpose: to give students of geology an introduction to the physics of the Earth and to give scientists in other fields some knowledge of geology and its relation to geophysics. The nature, composition and behavior of the Earth by integrating available information from geophysics, geology and geochemistry, are described in the seventeen chapters. There is a special emphasis given to the solid earth, although certain aspects of the physics of the upper atmosphere are included. The processes operating in the Earth and the nature of its interior, as revealed by the study of seismology and the physics of the Earth, have been used as a framework upon which to build a summary of the world's geology.

FRACTURE, edited by B. L. Averbach, D. K. Felbeck, G. T. Hahn and D. A. Thomas. 646 pages, diagrams, 6 × 9 in. New York, John Wiley & Sons, Inc.; Cambridge, The Technology Press; 1959. Price, $17.50.

Based on the proceedings of a conference on the atomic mechanisms of fracture held in Swampscott, Mass. in April 1959, this book is concerned with the question of what actually occurs on an atomic scale when materials fracture. Current theories are considered for the cleavage, ductile, fatigue, and high temperature mechanisms of fracture for metals, ceramics and polymers. One of the text's most valuable features is the delineation of future research needs. The conference was sponsored by several Government offices.

MAN-MADE TEXTILE ENCYCLOPEDIA, edited by J. J. Press. 913 pages, illustrations, 8¼ × 11 in. New York, Textile Book Publishers, Inc. (division of Interscience Publishers Inc.), 1960. Price, $27.50.

The editor's aim in preparing this encyclopedia was to integrate the many facets of the rapidly expanding man-made textile industry. Articles were contributed by 148 experts; each of twenty-one chapters deals with a different phase of the field. This comprehensive reference work covers everything from raw materials through processing and uses to the final chapter on "Economics and Statistics." Well-chosen photographs and carefully prepared tables add to the value of the book, which is certain to become the standard reference in the field.

NATURE AND PROPERTIES OF ENGINEERING MATERIALS, by Zbigniew D. Jastrzebski. 571 pages, illustrations, 5¾ × 9 in. New York, John Wiley & Sons, Inc., 1959. Price, $11.00.

Written from an engineering viewpoint, this book is intended as a text in basic courses in engineering materials. Prerequisites are general chemistry, general physics and some calculus. Subjects covered include the structure of matter, colloids, mechanical properties, metals (ferrous and nonferrous), thermal properties, corrosion, friction and lubrication, cements, and protective coatings.

CHEMISTRY OF NUCLEAR POWER, by J. K Dawson and G. Long. 208 pages, diagrams, 5½ × 8½ in. New York, Philosophical Library; 1959. Price, $10.00.

The authors, both of the Reactor Chemistry Group at Harwell, have painted a broad picture of the development and current state of the art in the field of nuclear power as it is related to chemistry. The book is aimed primarily at those who have had some scientific training, but it is not for specialists.

MEDICAL X-RAY TECHNIQUE, by G. J. van der Plaats. 480 pages, diagrams, 6 × 9 in. New York, Macmillan Company, 1959. Price, $10.00.

This text is concerned with the medical employment of X-rays in diagnosis and therapy, particularly for radiographers in training. Many points have been fully treated in order to ensure that the radiographer be well versed in the principles and varied applications of his subject. Included in the book is an extra chapter on radioactive isotopes, in view of the close relationship existing between medical X-ray technique and the radiological problems treated in the therapy section.

INTRODUCTION TO CHEMICAL ENGINEERING PROBLEMS, by William H. Corcoran and William N. Lacey. 185 pages, diagrams, 6 × 9 in. New York, McGraw-Hill Book Co., Inc., 1960. Price, $9.50.

This volume is aimed at preparing sophomore chemical engineering students for courses in physical chemistry and industrial chemistry and stoichiometry. Differential and integral calculcus and first-year college chemistry are the only prerequisites.

THEORY OF ELASTICITY, by L. D. Landau and E. M. Lifshitz. 134 pages, 6 × 9 in. Reading, Mass., Addison-Wesley Publishing Company, Inc., 1959. Price, $6.50.

Written primarily for physicists, this text includes not only the ordinary theory of the deformation of solids, but also some topics not usually found in other books on this subject, such as thermal conduction and viscosity in solids, and various problems in the theory of elastic vibrations and waves.

MANUAL OF MATHEMATICAL PHYSICS, by Paul I. Richards. 486 pages, diagrams, 6½ × 9½ in. New York, Pergamon Press, 1959. Price, $17.50.

This manual represents a concise source of information on theoretical physics and allied mathematical techniques. It contains a wide and comprehensive selection of formulae and statements which are either basic to physical theory or useful in a wide range of applications. The physics section is confined to well-established theories and the mathematical section omits material readily available in handbook form. The level of the treatment varies with the level of the material, but is always highly condensed and must therefore be considered advanced despite the inclusion of fundamental material.

LABORATORY TECHNIQUE IN ORGANIC CHEMISTRY, by Kenneth B. Wiberg. 262 pages, diagrams, 6 × 9 in. New York, McGraw-Hill Book Co., Inc., 1960. Price, $7.50.

A practical book designed to introduce advanced undergraduates and beginning graduate students to the basic methods of organic chemistry laboratory technique. The author covers the procedures and methods of purification of liquids, solids and gases, and considers the methods of separation between phases, such as chromatography and the techniques involved in carrying out a reaction. All important techniques have been covered and wherever possible, references are given to more detailed discussions of the methods.

STRUCTURE AND PROPERTIES OF THIN FILMS, edited by C. A. Neugebauer, J. B. Newkirk and D. A. Vermilyea. 561 pages, diagrams, illustrations, 8½ × 11 in. New York, John Wiley & Sons, Inc., 1959. Price, $15.00.

This book presents the complete proceedings of an international conference on thin films held at Bolton Landing on Lake George, New York in September 1959. Recognized experts in their specialized fields contributed review papers with discussions on problems recently solved as well as those still awaiting solution. Topics include the preparation, growth and structure of thin films, and the mechanical, electrical, optical, and magnetic properties of thin films. A review of processes occurring at the metal surface is also given, since a large fraction of thin film atoms resides on or near the surface.

EXPERIMENTS IN ELECTRONICS, by W. H. Evans. 374 pages, diagrams, 6 × 9 in. New York, Prentice-Hall, Inc., 1959. Price, $6.95.

This new text encompasses 100 carefully planned experiments. Taking into account the problem of availability of equipment, the choice of experiments is geared to easily obtained laboratory materials. Two experiments are given on each subject, which include: cathode-ray oscillographs, thermionic emission and space charge, Q-measurements, doubly tuned radio frequency amplifiers, frequency conversion, and R-C transients. A wide range of transistor experiments is included employing both vacuum tubes and transistors and solid state devices.

ATMOSPHERIC DIFFUSION AND AIR POLLUTION, edited by F. N. Frenkiel and P. A. Sheppard. 471 pages, diagrams, 6 × 9 in. New York, Academic Press Inc., 1959. Price, $12.00.

The sixth volume in the "Advances in Geophysics" series contains the proceedings of a symposium held in Oxford in August, 1958. In contrast to the previous volumes, this one is devoted to a single subject, with 47 contributors concentrating on diffusion problems. In this way, the editors hope that progress in many areas of geophysics will be stimulated, for example, atmospheric electricity, cloud physics, suspension of radioactive substances and ozone distribution.

THE FOUNDATIONS OF CHEMICAL KINETICS, by Sidney W. Benson. 703 pages, diagrams, 6 × 9 in. New York, McGraw-Hill Book Co., Inc., 1960. Price, $13.50.

The author's aim in writing the text was to present a broad survey of the entire field of kinetics, so that the senior or graduate student would be acquainted with the basic models and language in current use. In addition, several sections go into detail about more specialized phases such as the kinetics of gas reactions relative to solutions and solids, and the mechanism of complex gas-phase reactions.

CHEMICAL ANALYSIS, by Herbert A. Laitinen. 611 pages, diagrams, 6 × 9 in. New York, McGraw-Hill Book Co., Inc., 1960. Price, $12.50.

This text for advanced undergraduate and graduate students emphasizes the fundamental principles of analytical chemistry. It can also serve as a reference work for practicing analytical chemists.

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PUBLICATIONS RECEIVED

THE ELECTRIC ARC, by J. M. Somerville. 150 pages, diagrams, 4 × 6½ in. Methuen's Monographs on Physical Subjects. New York, John Wiley & Sons, Inc.; London, Methuen & Co., Ltd.; 1959. Price, $2.50.

MASERS, by Gordon Troup. 168 pages, diagrams, 4 × 6½ in. Methuen's Monographs on Physical Subjects. New York, John Wiley & Sons, Inc.; London, Methuen & Co., Ltd.; 1959. Price, $2.75.

DE MAGNETE, by William Gilbert, translated by P. Fleury Mottelay. Unabridged and unaltered republication of the translation published in 1893. 368 pages, diagrams, 5½ × 8 in. New York, Dover Publications, 1958. Price, $2.00 (paper).

TABLES NUMERIQUES DES FONCTIONS ASSOCIEES DE LEGENDRE, calculated by Centre National d'Etudes des Telecommunications. 639 pages, 8¼ × 11¼ in. Issy-les-Moulineaux (France), C.N.E.T., 1959. Price, 56 N. F.

FEDERAL FUNDS FOR SCIENCE. VIII. THE FEDERAL RESEARCH AND DEVELOPMENT BUDGET, FISCAL YEARS 1958, 1959 AND 1960, prepared by the National Science Foundation. 74 pages, charts, 8 × 10 in. Washington, Government Printing Office, 1959. Price, $0.45.

ROUTE SURVEYS AND DESIGN, by Thomas F. Hickerson. Fourth edition of a book previously published under the title "Route Location and Surveying." 568 pages, diagrams, 4½ × 7 in. New York, McGraw-Hill Book Co., Inc., 1959. Price, $8.50.

A GUIDE-BOOK TO BIOCHEMISTRY, by K. Harrison. 150 pages, diagrams, 5½ × 8½ in. New York, Cambridge University Press, 1959. Price: $3.50 (cloth); $1.95 (paper). This is the first text to appear in Cambridge University Press's new series designed to bring textbooks within easy reach of the student. Cloth and paper editions of a number of new texts will be issued simultaneously.

VALENCY, CLASSICAL & MODERN, by W. G. Palmer. Second edition, 244 pages, diagrams, 5½ × 8½ in. New York, Cambridge University Press, 1959. Price, $5.50.

CRYSTALS AND CRYSTAL GROWING, by Alan Holden and Phylis Singer. 320 pages, illustrations, 4½ × 7½ in. New York, Doubleday & Company, 1960. Price, $1.45 (paper) ($1.65 in Canada).

THE PHYSICS OF TELEVISION, by Donald G. Fink and David M. Lutyens. 160 pages, diagrams, 4½ × 7½ in. New York, Doubleday & Company, 1960. Price, $0.95 (paper) ($1.10 in Canada).

WAVES AND THE EAR, by Willem A. Van Bergeijk, John R. Pierce and Edward E. David, Jr. 235 pages, diagrams, 4½ × 7½ in. New York, Doubleday & Company, 1960. Price, $0.95 (paper) ($1.10 in Canada).

THE BIRTH OF A NEW PHYSICS, by I. Bernard Cohen. 200 pages, illustrations, 4½ × 7½ in. New York, Doubleday & Company, 1960. Price, $0.95 (paper) ($1.10 in Canada).

Editor's Note: The four books above are in the new Science Study Series published by Anchor Books, an affiliate of Doubleday & Company; they have been prepared under the direction of the Physical Science Study Committee of Educational Services Incorporated.

HOW TO GET THE MOST OUT OF YOUR VOM, by Tom Jaski. 224 pages, illustrations, 5½ × 8½ in. New York, Gernsback Library, 1960. Price, $2.90 (paper).

RADIATION COUNTERS AND DETECTORS, by C. C. H. Washtell. 115 pages, illustrations, 5½ × 8½ in. New York, Philosophical Library, 1960. Price, $7.50.

THE STORY OF DISSECTION, by Jack Kevorkian. 80 pages, 5½ × 8½ in. New York, Philosophical Library, 1959. Price, $3.75.

MEDIEVAL AND RENAISSANCE MEDICINE, by Benjamin L. Gordon. 843 pages, plates, 5½ × 8½ in. New York, Philosophical Library, 1959. Price, $10.00.

IGNEOUS AND METAMORPHIC PETROLOGY, by Francis J. Turner and John Verhoogen. Second edition, 694 pages, diagrams, 6 × 9 in. New York, McGraw-Hill Book Co., Inc., 1960. Price, $12.00.

POWER STATION ENGINEERING AND ECONOMY, by Bernhardt G. A. Skrotzki and William A. Vopat. Second edition of "Applied Energy Conversion," 751 pages, illustrations, 6 × 9 in. New York, McGraw-Hill Book Co., Inc., 1960. Price, $12.50.

THE NATURE OF THE CHEMICAL BOND, by Linus Pauling. Third edition, 644 pages, diagrams, 6 × 9¼ in. Ithaca, Cornell University Press, 1960. Price, $8.85.

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Remote Control Fallout Sweeper.—A remote-controlled decontamination sweeper designed to keep Air Force facilities operational in spite of radioactive fallout is being tested at the Air Research and Development Command's Air Force Special Weapons Center, Albuquerque, New Mexico.

Equipped with a radio guidance system and three television cameras, the sweeper can clean a seven and one-half foot path of radioactive debris at one pass. Anticipated cleaning efficiency is more than 99 per cent.

Built by the G. H. Tennant Company of Minneapolis, Minn., the 10-ton unit can perform double duty as a decontamination sweeper in case of attack and as a manually-operated cleaner for everyday routine ramp, runway, taxiway and hangar sweeping.

Remote operation is controlled from a duplicate of the actual manual control panel. The operator, located in a protected position and using the radio guidance system, can steer, shift gears, move the throttle, operate the brakes and empty the sweeper's storage hopper. He can see surrounding terrain and the instrument panel on television screens and hear the sound of the engines through an open microphone.

Using the television system, the operator can carry out a damage survey in conjunction with the clean-up operation. The cameras can be rotated exactly as the human head is turned so that all functions performed by the operator are as natural as possible.

Radiation detection equipment is being developed for use on the sweeper to provide an indication of the intensity of radiation before and after decontamination is completed.

The sweeper, measuring 15 ft., 3 in. long and 9 ft., 9 in. high, is powered by two 93-hp. Hercules engines. One engine operates the propelling system and hydraulic pumps; the other operates the main brush, vacuum system, jet blower, and generator for the electronic system.

Current tests, conducted by the Special Weapons Center's Biophysics Division, will determine the accuracy of remote control at distances up to and exceeding five miles, cleaning efficiency at various speeds, and the characteristics and capabilities of mechanical and electronic components. The tests will be conducted under non-radioactive conditions.

Photopolymer Printing Plates.—Plans to build a full-scale plant to produce "Dycril" photopolymer printing plates have been announced by the du Pont Photo Products Department. The manufacturing unit will be built on the site of the company's Parlin, N.J., photo products plant. When completed in mid-1961, it will have an initial capacity of one million square feet of printing plates per year. Meanwhile, to supply the growing demand for these unique photosensitive plates, the capacity of the existing pilot-scale manufacturing facilities at Parlin will be doubled.

Du Pont scientists discovered in 1949 the possibility of making printing plates by using a plastic material which hardens when exposed to light.

After years of painstaking research and development work at a cost of more than $6 million, commercially acceptable photopolymer plates were developed and introduced to the trade in 1957. Intensive field evaluation work led to the decision to build a plant to produce the plates in quantity.

Polymers are compounds containing huge chains of atoms assembled from simpler molecules which the chemist calls monomers. Because of their interlocking construction, size, shape and weight, many polymers are characteristically tough and durable. Du Pont research was successful in perfecting a plastic monomer which could be converted into a polymer, or "polymerized," by simple exposure to ultraviolet light. This material is known as a photopolymer.

A "Dycril" photopolymer printing plate is made up of a thin layer of unexposed plastic bonded to a metal support. To produce a printing surface the blank plate is covered by a high-contrast photographic negative containing the matter to be reproduced. The plate is then exposed through the negative to an ultraviolet light source, such as a carbon arc. Light striking the intended printing areas causes the polymerization process to take place, making these areas of the plate hard and relatively insoluble. The unexposed, and therefore unaffected, plastic does not harden and is removed by a pressurized water spray containing less than one per cent sodium hydroxide. This leaves the printing areas in relief. A ready-to-run photopolymer plate can be made from a negative in 15 to 20 minutes.

One result of the process is that whatever is in the negative will be reproduced exactly in the photopolymer plate. This makes it essential that the negative meet high standards and rather precise requirements. The negative should be clear and sharp and free of any foreign matter or imperfections. For best results, the highlight areas should measure no less than five per cent (for a 120-line screen) and shadow areas should not be heavier than 85 per cent. The emulsion side of the negative must have a light matte finish and the negative must be developed to a background density of 3.5 or higher. To assure proper registration for color plates and the accurate size of all copy, film on a dimensionally stable base, preferably polyester, should be used.

Based on tests to date, the use of "Dycril" plates for process color appears to present no particular problems although more research is planned in this area. Platemaking for multi-color printing via the photopolymer system is expected to be less costly than photoengraving since color masking corrections can be applied in producing the negatives for the "Dycril" plates. Du Pont is also continuing research and development work in the area of matting and molding and the use of flexographic inks.

Two relatively new developments in printing appear particularly suitable for the use of "Dycril" photopolymer printing plates. One is the dry offset process which combines features of both lithography and letterpress and has many theoretical advantages, but the absence of a suitable printing plate has limited its use. "Dycril" has worked well in tests and one printer is planning a commercial installation using these plates. There are about 500 installed offset presses which can accept a thin (.030-in.) photopolymer plate and print by this process.

The other development is a new

high-speed rotary letterpress which uses wrap-around plates. These presses do away with the time-consuming make-ready now needed to obtain common printing height and proper registration with page-size electrotypes. "Dycril" photopolymer printing plates have been used experimentally on prototype high-speed presses introduced by two manufacturers, although the present standard-size photopolymer plates are too small for this general commercial use. However, by mid-1960 du Pont plans to have a larger, 24 × 28 in., plate and by 1961 a full-size 39 × 76 in. flexible plate with a .015-in. relief designed for this use.

There are many other potential uses for photopolymer plates in both printing and non-printing fields which du Pont plans to investigate. "Dycril" might be used to make gravure, or recessed, printing plates by the use of a positive, instead of a negative, transparency. Non-printing specialty applications include such things as signs, emblems, plaques, embossing dyes, and printed circuits.

Transistorized Fuel Gage.—A transistorized atomic energy gage which measures the amount of fuel in airplanes and missiles with greater accuracy and reliability than conventional devices has been developed for the U.S. Navy by Atomics International, a division of North American Aviation, Inc.

In a series of tests conducted by North American's Autonetics Division, the gage was installed in a twin-engine jet bomber and environmentally tested at altitudes to 38,000 ft. The light-weight radiation fuel gage performed accurately during all flight altitudes. Cosmic radiation or changes in altitute did not affect the performance of the gage. It is scheduled for additional testing by the Navy.

All types of solid and liquid propellants can be measured by the gage. Performance of the gage is not affected by impurities in the fuel. It measures all the fuel, is easily installed and automatically accounts for differences in hydrocarbon, or petroleum-based, fuels which affect the accuracy of conventional devices.

It consists of gamma radiation sources, such as Cobalt-60, scintillation detectors containing a ratemeter and pulse amplifier, a power supply and an indicator.

Radiation sources and detectors are mounted on the sides of each fuel tank. Gamma rays given off by the radioactive materials decrease in intensity as they pass through the fuel.

The amount of fuel is determined by the intensity of the gamma rays reaching the detectors. The ratemeter converts pulses picked up by detectors into d-c voltage and the quantity of fuel is recorded in pounds by the indicator.

Calibration of Aircraft Compass Transmitters.—A compact device for quick and accurate calibration of gyro-stabilized compass system transmitters has been announced by General Electric. Unusual design of the device—called a rictometer—permits it to eliminate more than 99 per cent of the Earth's horizontal magnetic field, while creating a known horizontal field adjustable through 360 deg.

Because it also shields out other external magnetic fields, company engineers said the device can be used in any shop or laboratory location without regard for magnetic disturbances caused by moving vehicles, elevators, electric wiring, and other unavoidable variables.

An important feature of the rictom-

eter, the engineers explained, is its relatively low cost. They said it can be purchased and installed at a fraction of the cost involved in building and locating the Helmholtz coil system it is designed to replace.

Applications for the new device are foreseen by General Electric in aircraft and aircraft instrument plants, commercial aviation overhaul shops, aircraft carriers, and military airfields.

The engineers said that with its current off, the rictometer simulates the Earth's magnetic field at a conservatively estimated altitude of 35,000 miles. They said this suggests its use in calibrating space-bound magnetometers as well as aircraft compass transmitters.

About one foot in diameter and roughly spherical in shape, the rictometer consists of three pairs of Helmholtz coils surrounded by a system of shields. Interior of the instrument is accessible by raising a hinged lid. A special mounting arrangement within the coils accepts the transmitter to be tested quickly and easily.

Four concentric shields form the rictometer's shell. Two of these, the inner and outer shells, are of nonmagnetic steel and serve as structural elements. The other two, mounted between the steel shells, are fabricated of a high-permeability metal that diverts external magnetic fields from the instrument's interior.

With this arrangement, Company engineers said, a typical horizontal earth field of 200 milligauss is reduced to about 0.32 milligauss within the shell.

In operation, the two Helmholtz coil systems within the shielding system restore the horizontal component of the Earth's field with a directional precision of ±12 min. Compass transmitter calibration is performed by altering the currents between these coils, which rotates the horizontal component in known increments through 360 deg.

Testing the rictometer's accuracy in an externally induced d-c. field of 50 gauss, engineers at the Company's Instrument Department, West Lynn, Mass., reported a maximum transmitter calibration error of 0.15 deg. They said this indicated the instrument could be used to calibrate compass transmitters accurately even though placed within a foot of a 10,000 amp. d-c. bus.

Because it shields out external magnetic influences, the rictometer eliminates multiple average readings necessary with commonly used Helmholtz coil systems. Time required for calibration of a single compass point will be not more than two minutes, as opposed to 15–30 min. commonly needed with Helmholtz systems.

Standard power requirement for the rictometer is 24 volts d. c. Company engineers said that its low current requirements permit use of a simple battery power supply, with only infrequent replacement required.

Nose Cone "Bends" to Study Solar Activity.—A revolutionary nose-cone which automatically "bends" while speeding through outer space to aim complex solar measuring instruments at the sun has been successfully test-flown, according to the Air Research and Development Command (ARDC). The launch was made from the White Sands Missile Range, New Mexico.

Labeled "Solar Pointing Control," the cone, attached to an Air Force Aerobee-Hi research rocket, soared 90 miles above the Earth recently to make detailed studies of the sun.

As the rocket reached its peak altitude, the 8-ft., 2-in. long cone bent at a 90-deg. angle from its axis to point

its cargo of electronic equipment at the sun. As soon as its observations were completed, the cone reverted to its original position for a parachute descent to Earth.

The scientific equipment was contained in the upper 6-ft., 5-in. section of the cone. Included in the package was an X-ray spectrograph developed for ARDC's Air Force Cambridge Research Center (AFCRC) by Baird Atomic of Cambridge, Mass. Photo sensors and servomechanisms providing guidance and electronically-programmed power for the bending operation were in the cone's 1-ft., 9-in. base.

Previous pointing control systems utilized a static, non-flexible configuration. The maneuverability of the new design permits a highly increased equipment payload.

The cone was developed for ARDC by Ball Brothers Research, Inc., of Boulder, Colo.

New Scintillation Detector Preamplifier.—Nuclear-Ohio, Inc. of Bay Village, Ohio, has announced that production has commenced on a new line of scintillation detector preamplifiers to be known as the "V" line. These improved preamplifiers have been designed to provide the utmost versatility and may be used with a wide variety of crystal detectors including the "Standard" Nuclear-Ohio crystal assembly, the Harshaw "Integral Line," and others.

Two features of the preamplifier include a focusing control to adjust the screen voltage for optimizing resolution, and high voltage filtering in the dynode voltage divider. The preamplifier, when combined with a suitable detector assembly, may be used for pulse height analysis or integral counting.

The "V" preamplifier is normally provided with a threaded ring assembly which easily and rigidly connects the preamplifier and crystal detector assembly. When quality crystal detector assemblies such as the Nuclear-Ohio "Standard" or the Harshaw "Integral Line" are connected to the "V" preamplifier, highest resolution, efficiency and versatility are assured.

Specifications for the "V" preamplifiers include: Input power requirements; 175 milliamperes at 6.3 volts a.c., 5 to 7 milliamperes at 150 to 250 volts d.c. High voltage requirements: adjustable 500 to 1500 volts positive d.c., 0.25 milliampere current at 1000 volts. Low current voltage dividers are also available.

Cables to 10 ft. in length are normally supplied, but other lengths may be specified. Connectors for any scaler or spectrometer will be factory installed to customer's requirements. Performance is guaranteed with a one year warranty. Accessories are available for mounting complete scintillation detectors.

Halogen Leak Detector for Contaminated Atmospheres.—For locating refrigerant leaks when the atmosphere may be heavily contaminated with halogen gas, a portable electronic leak detector equipped with a special probe is available from General Electric.

Engineers at the company's Instrument Department, West Lynn, Mass., said the new leak detector kit will be useful in locations where concentrations of halogen compounds such as refrigerants, paints and varnishes may be present in the air. Typical uses would be during the installation of large commercial and shipboard air conditioning systems, and in checking for refrigeration leaks in confined, unventilated devices such as vending machines.

The kit consists of a low-cost Type H-6 leak detector equipped with a newly designed proportioning probe, and a Type LS-20 leak standard. The equipment is packaged in a compact leather case for portability and protection.

The probe works this way: by twisting an adjustment at its tip, the operator can mix varying amounts of pure air—cleaned by a built-in filter—with the air being drawn through the probe. This "dilutes" the air being tested so that halogens present in the atmosphere will not be detected. The leak detector, however, remains fully sensitive to all refrigerant leaks of ½ oz. a year and larger.

Leak detectors equipped with the new probe operate accurately in areas where air contamination is ten to twenty times above the normal limit for standard models, company engineers said.

The leak standard included with each kit is necessary for calibrating and checking detector sensitivity, and for use in determining the size of leaks.

Company engineers said that plants and service shops already using General Electric Type H-6 leak detectors may purchase proportioning probes as accessories. In this case, they pointed out, it will be necessary to have on hand a Type LS-20 leak standard to assure accurate calibration of their equipment.

"Twistors" for Electronic Memory Devices.—Computer technology has been advanced by a new, accurate and fast electronic memory, developed by Western Electric and Bell Telephone Laboratories.

Hair-thin wires wrapped with magnetic metal tape in barber-pole fashion plus tiny bar magnets are the basic elements of the permanent electronic memory which is comparatively inexpensive, easy to produce, and will help speed up computer developments. The memory will supply bits of information at speeds in millionths of a second to a computer.

Only 14 months elapsed between the decision to use the memory and its readiness for mass production. This was accomplished through the closely coordinated research-engineering work of Bell Laboratories and Western Electric.

A typical memory unit is built up of alternating grids of magnetic wire strands called "Twistors" and plastic cards with arrays of minute bar magnets. The Twistor is a thin, hair-like copper wire, around which is spirally wrapped a magnetic tape $\frac{1}{10}$ of the thickness of the wire itself. The magnet array is formed by bonding a thin sheet of magnetic material to a plastic card and then removing all undesired magnetic material by precise photoetching methods. The finished card stores information in much the same way as a punched card in an office calculator or computer.

In a punched card, information is stored by the presence or absence of a hole. Similarly, the new Magnet-Twistor memory retains information by the presence or absence of a bar magnet at a specific location on the plastic card.

When a punched card is inserted into a calculator, an electric current flows where the card is punched and does not where there is no hole, providing information to the calculator. When a Magnet-Twistor memory card is read by a computer, an electric pulse is transmitted if there is no magnet or blocked if there is a magnet present.

These electric pulses represent commands to the computer, telling it how to proceed in solving problems with accuracy and extreme speed of operation.